

Reports

An Interview with Adam Kuper

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[*Introduction:* In August 1999, Adam Kuper, a professor of anthropology at Brunel University, was in Brazil at the invitation of the Graduate Program in Social Anthropology (National Museum, Federal University of Rio de Janeiro) to give a series of lectures on his recently published book *Culture: The Anthropologists' Account* (1999b). In this interview with him, granted us on that occasion, we explore the connections between the social history and politics of his native South Africa and the intellectual development of anthropology from his perspective. The interview first appeared in Portuguese in *MANA: Estudos de Antropologia Social* 6(12):157-73.]

CF: Let us start with South Africa—the beginning of South African anthropology and your beginning as an anthropologist.

AK: Anthropology in South Africa was an anthropology that developed very much engaged with particular local political issues. This anthropology was very important in setting at least part of the agenda of modern British social anthropology through the channels of [A.R.] Radcliffe-Brown, who taught many years in South Africa and established the first chair there, [Bronislaw] Malinowski, who visited South Africa and many of whose students were involved in South Africa, and South African anthropologists who migrated to Britain. But there is a second history that is not so well known outside South Africa, and this is the history of Afrikaans-speaking anthropology, which was pro-segregation and which generated much of the scientific and institutional support for the apartheid programme. When I was an undergraduate in South Africa, there were these two kinds of anthropology—one very well known, very cosmopolitan, very tied to British scholarship, and this Afrikaner anthropology that was unknown outside South Africa but extremely powerful within it. In the English-speaking universities, the anthropology we studied was an opposition anthropology, very much cast as a critique of apartheid and of the kind of anthropology on which apartheid was based—which was a romantic, primitivist kind of

anthropology that insisted on treating South Africa as a single society, a society in rapid transition. Its characteristic ethnographic objects were such things as labour migration, mixed Christian-African religions, urbanization, and so on.

FN: Was it in this intellectual atmosphere that you studied anthropology?

AK: I studied anthropology for two years in South Africa as part of a degree whose main subject was history. Then in 1962 I left South Africa to spend nine months in Paris and then went to Cambridge as a Ph.D. student. So a major part of my anthropological education was not in South Africa, but it was my experience in South Africa which made me want to become an anthropologist, and it was the situation of South African anthropology which gave me the model for the kinds of problems and theoretical approaches I was interested in. Of course there was the fact that my aunt, Hilda Kuper, was an anthropologist, and she was a student of Malinowski's, and when I was 18 she took me into the field in Swaziland. She was a very bad driver, and the University of Natal would not allow her to drive the university Land Rover into Swaziland because she always had accidents. So she said, "Well, I have this nephew who is 18, and he is an excellent driver." So I went and drove her around. There was one encounter in particular which was very important. We were at the Queen Mother's village, where my aunt had done a lot of fieldwork, and as a man I could not go in, so I was sent to stay with the regiment that was guarding it. The prince in charge of this regiment, who was my age and dressed in classic Swazi clothes, with spear and so forth, took me into a beehive hut, where the soldiers brought a beer pot. He spoke perfect English, because like many of the Swazi aristocrats he had been educated in an English public school, and at one stage he asked me, "Do you believe in witchcraft?" I was 18 and had just stopped believing in all sorts of things, and suddenly I did not believe in witchcraft, so I said no. And he looked at me with pity and gave me this answer, which I have always thought about: He quoted *Hamlet*—"There are more things in heaven and earth, Horatio/Than are dreamt of in your philosophy." So I thought, "That is very interesting as a counter by a Swazi prince to the question of witchcraft and rationality." I thought, "Maybe I should become an anthropologist."

CF: And at that moment what was the power balance between English-oriented and Afrikaner anthropology?

AK: The Afrikaner anthropology had the government patronage. It was their graduates who went to work in the

Bantustans as administrators, in the civil service, in education, and they were the ones who were designing the plans for government administration. Institutionally they were completely in control. However, they knew that internationally they were pariahs, and they were very suspicious of the anthropologists in the English universities who had international reputations, who were publishing in the best journals, who were invited to give lectures here and there. It was a very bitter paradox that those who had power at home had no cultural capital internationally and vice versa. But the English anthropologists gradually began to leave. Not all: some felt a kind of moral commitment and would never leave. For example, Monica Wilson, who was the daughter and granddaughter and I think great-granddaughter of missionaries in the Transkei, had grown up there, and she was going to stay there.

FN: Were there institutional spaces for anthropology in those years in universities, federal agencies? Where were the anthropologists?

AK: It depended on which side of the divide you were. On the English-speaking side there were no opportunities except in the English-speaking universities. The most famous were Witwatersrand and Cape Town and then the University of Natal at Durban, where my aunt was a professor. And then there was a small university called Rhodes University in Grahamstown, where Philip Mayer had a research base doing very interesting urban studies. On the Afrikaans-speaking side, the students went into government, because they had control over the national museums and the provincial museums. It was very interesting when they began to crack. I was in South Africa in 1982 on a private, family visit, and I had a telephone call from the professor of ethnology at the Rand Afrikaans University. All the professors of ethnology in the Afrikaans universities were members of the Broederbond, the secret society at the heart of the Afrikaner establishment (Broederbond means “society of brothers”). In this telephone call, the chap said, “I know you are here for a personal visit, but if you have time I would very much like to talk to you.” So I went to visit him, and it was the first time I had set foot in this university, which was built about two kilometres from my university, Witwatersrand University, by the government as a challenge to this liberal, leftist university. This apartheid university was built like a fortress. It was terrifying walking in; it was real fascist architecture—you felt tiny. And I walked along the corridor to the professor of ethnology’s office, and on the other side of the corridor was the Department of Research in National Security. I walked into the anthropology professor’s office, and he started talking passionately. As soon as he realized that I understood Afrikaans he switched to Afrikaans and told me that he was doing fieldwork in Gazankulu, an extremely poor part of the country; he spoke the language very well. And the more he did fieldwork the more he realized that apartheid, far from helping these people, was actually a main cause of their problems. His first

reaction was to try to explain this to the people in Pretoria, and so he began talking to people, and of course he realized that they did not want to hear. And he began to be ostracized by his colleagues; his children at school began to have problems with the authorities, and people in Pretoria, the government agencies, began telling him to watch his step. And eventually the only people he could talk to were the two colleagues who were doing research with him. So he called me just to pour out his heart.

CF: What was the relationship between Afrikaner and English-oriented anthropology?

AK: There were two anthropology associations, the English-language one, which of course also had the black anthropologists, and the Afrikaans-language one. And if any of the members of the Afrikaner association came to the English-language meeting, they were in real trouble with their bosses. The next year the professor who had called me came to the meeting of the English-language association, and when he was about to give his paper a government official stood up and said that he could not give it because the material had been commissioned by the government. It was very dramatic. About a year later, I was at the English-language conference, and there was one young Afrikaner anthropologist there, which was very unusual. And I sat up with him one night talking and drinking brandy—South African brandy, which is very good and very cheap—and he started telling me a story. He was born in Potchefstroom, an Afrikaner town in the Transvaal which is famous for being extremely Calvinist. He grew up there, went to the university to study anthropology, and did the research for his M.A. on the coloured community in the town. As people got to know him, they said, “Look, we used to have these houses and these shops in this part of town, and the city council appropriated it all and kicked us out, and one of the people was your uncle. He then bought the land very cheap.” So he went to the city council archives, and it was true. He began to question himself and distance himself. It was very interesting at this time, as these Afrikaner anthropologists—very honest people, many of them, but very provincial and close in to a community which was very loyal, highly hierarchical, and disciplined—began gradually, one by one, to break out.

CF: Do you know the origin of the Broederbond?

AK: I’m not sure. It was when the nationalist movement was really getting started in the 1920s. This was a network of elites—headmasters, Calvinist ministers, lawyers, politicians, and professors—that eventually became extremely important within the party and within the government.

FN: When were the universities in South Africa founded?

AK: The University of Cape Town is the only one that

goes back to the end of the 19th century. The University of Stellenbosch started, I think, in 1910, the University of Witwatersrand in the 1920s. My mother was one of the first students, and I think she was there in the second year of the university, so it was about 1921 or 1922.

CF: What did she study?

AK: Mathematics. She was very friendly at university with Eileen Krige, who, with her husband, J. D. Krige, coauthored one of the classic South African ethnographies, *The Realm of a Rain Queen* [1943], on the Lovedu of the Transvaal. My aunt Hilda Kuper knew her, and when Malinowski came to South Africa he came to my house. (This was before I was born.) And so when I said I wanted to become an anthropologist, my mother knew what it was about, and she was really angry! She said, "You're completely crazy!" My father's brother was Leo Kuper, who was a very famous sociologist in South Africa, and Hilda Kuper was his wife. During the war, when I was a small boy, my uncle was in the army in North Africa and then in Italy, and my aunt Hilda Kuper lived with us, and so I bonded very much with her.

FN: How did she become an anthropologist?

AK: I'm not sure. There was a woman who taught anthropology at the University of Witwatersrand—the first person to teach anthropology there, Agnes Winifred Hoernlé, both in South Africa in 1885. She studied in Cambridge with Haddon and Rivers, then in Leipzig with Wundt and also for a bit with Durkheim in Paris, and married a German professor of philosophy. They were at Harvard together when he became ill, and in those days if you had lung disease they often sent you to Australia or South Africa. So they came to South Africa, and he became a philosophy professor at the university and she started the anthropology department there. She was an extremely charismatic teacher. The anthropology she taught was an anthropology that was very engaged politically, very critical of South Africa's race relations policy and focused on what many people saw as the new social problems, urban problems. There was no sociology to speak of, no political science, so for young students at the university there was this charismatic teacher teaching something that was relevant. And the students themselves, of course, were very often politically aware; this was the time of the rise of fascism in Europe, and there was a feeling that there was a danger in South Africa of some kind of fascist system. For these kinds of students, Mrs. Hoernlé's anthropology was a very interesting thing, and I think Hilda came into that. She, in fact, was not South African; she was from Rhodesia and had come as a student. And then [Isaac] Schapera had just come back; he was the first one to be sent to study with Malinowski, and he encouraged some of the others. And that was the very exciting period around Malinowski, of course.

CF: Who were the first South African anthropologists?

AK: Well, there is the early period with missionaries and administrators, some of whom were very distinguished anthropologists, such as Henri Junod, who wrote classic African ethnographies and when he retired from the missionary service became the professor of ethnology at Neuchâtel. They were people whose expertise was long involvement with the area, knowing the language very well, often writing grammars and dictionaries as well as ethnohistories and ethnographies. But South African anthropology took an extraordinary turn when the first chair of anthropology was set up at the University of Cape Town in 1921 and, by a series of accidents, was given not to a South African but to Radcliffe-Brown, the pioneer of a new sociological and comparative anthropology. And he came and set this up, turning his back on the established South African ethnography and attempting to create a completely new discourse in South Africa. He was not just doing this in the abstract; he was attempting to establish an anthropology in South Africa which addressed South African concerns, and he was forced to recognize the big debate between the segregationists and the antisegregationists. He incorporated this into his anthropology, and among other things I think that it made him very critical of the kind of culture theory that Malinowski and the American anthropologists were developing. So English-speaking anthropology in South Africa was from the first on the cutting edge of British anthropology, of international anthropology of that kind. The first generation of leading students was sent to study with Malinowski and come back to reinforce these exciting new ideas. The Golden Age of South African anthropology was the 1930s and 1940s, when the great ethnographic monographs were published by people who had been studying in England with the major anthropologists and had come back regarding themselves as part of this Malinowskian revolution in anthropology. People like Max Gluckman, Hilda Kuper, Monica Wilson, and Ellen Hellman were part of this very modernist anthropology, this newly professionalized anthropology. And in a way what happened to it was that, with the triumph of Afrikaner nationalism after the war, the triumph of apartheid and the loss of political status of the English-speaking universities, these people and their descendants left South Africa. So English-speaking South African anthropology tended to stagnate after the 1950s and 1960s.

CF: What is striking to me is that at the beginning of the 1930s the seminars held by Malinowski had more or less 20 people, a quarter of them South African. Why did South Africa export so many anthropologists?

AK: Well, because in South Africa the political question was essentially a question of, however it was defined, race, ethnicity, or culture, and politically aware people or simply people who wanted to do something socially useful would be involved in anthropology in a very specific way. However, it was also evident to some extent that the kinds of issues that were being faced in South Africa were similar to some of the issues that you found

in other British African territories. So there was this interest on both sides in South Africa as the most advanced, most complicated example of a certain kind of industrializing African society.

FN: Probably it also has something to do with the singularity of South Africa in the context of the British Empire.

AK: Well, where else did you have anthropologists coming from? Australia was also sending anthropologists to Britain, and so was New Zealand and to a certain extent Canada. South Africa was probably sending more, but it was in a similar position. In these quasi-colonial situations, postgraduate training still had to be done in the metropolis, in all fields.

FN: Could you tell us something about your migration to England? Had you been in England before?

AK: I'd been; my father had taken me and my brother on a holiday in England for about a week. But I knew that I had to leave South Africa. This was just after a series of extremely repressive actions by the government in the 1960s. Sharpeville was a major confrontation in which a number of unarmed black people were shot—a state of emergency, when a large number of people were arrested and imprisoned indefinitely without trial. It became obvious to many of us that the situation was desperate and that if you remained in South Africa it involved a very long-term complete political commitment or a very compromised existence. So I decided to leave, but I wanted to make my life in Africa. I went to Cambridge to do my Ph.D. I wasn't allowed to do research in South Africa; I went to Bechuanaland, which was next to it. When I finished my Ph.D. in 1966 and got married, my wife and I went back to Bechuanaland for a while and then to Uganda, where we lived for three and one-half years while she did her research for her Ph.D. and I taught at the university. I would have happily remained there indefinitely; I was beginning research there as well. Then it became clear that the political situation there was becoming critical, and in fact Idi Amin's coup was only a few months later. There were three vacancies in social anthropology: in Singapore, in Hong Kong, and in University College London. I applied for them all, hoping to go to Hong Kong or Singapore. The only job I was interviewed for was London, and I was given the job, and so we came back to London in 1970.

CF: In which college did you study in Cambridge, and with whom did you take classes?

AK: In King's College. The two senior people in the college, and indeed in the department, were Meyer Fortes and Edmund Leach. I've often told the story—I even published it—about my first two days there. On the first day Edmund Leach took me for lunch and said—this was a very small department, there were about six members of the faculty, and I already knew there were two fac-

tions—"Look, there are two main groups in the department. There is my group, where people work in Asia, and the other group, which works in Africa. What about you?" And I said, "Well, I'm going to work in southern Africa." So he had no further interest in me but was polite enough to keep on talking. He said, "Let me tell you something about Cambridge. Meyer Fortes"—who was another South African Jew, by the way—"has been here many years, and of course knows a great deal about the Tallensi in Ghana, but he has never really understood Cambridge. And the thing you must understand about Cambridge is this: it is essentially lower-middle-class." And I said, "Thank you very much." The next day Meyer Fortes took me for lunch. He said, "So you're going to work in Africa? Where?" I said, "Well, I think in southern Africa." He said, "All right, we'll go and talk to Schapera in London." And he said, "Let me tell you about Cambridge. Edmund, of course, has been here for a long time, and he wouldn't tell you this; he probably doesn't even know this. The thing you must remember about Cambridge is they don't like Jews." And both these statements were true, and they were useful. But I learned practically no anthropology at Cambridge; there was no instruction, nothing. You just read and you went to the seminars, a few lectures, which were usually very boring. And then I went and did my fieldwork, I wrote up my Ph.D., and I got my first job. Then I started learning anthropology, because when I went to Uganda in 1967 I had to teach the introduction to social anthropology, and I knew nothing about it.

FN: What about your first fieldwork for your Ph.D.?

AK: My first and second fieldwork were both in the Kalahari, with a group of people whom Schapera had spoken to and suggested that I should go and study, very far away in the Kalahari desert, very isolated. And so I studied them for my Ph.D., and then I went back for another nine months before I went to Uganda. This was a Bantu-speaking pastoral people, and I was interested in the political system. In the back of my mind was the question of whether if you went to a very isolated southern African tribe you found what the Afrikaner anthropologists were talking about—this kind of viable tribal society with a chief, and so on. I found that even there, in that very isolated area, village politics, tribal politics was completely penetrated by the national structure of that very weak colonial state, Bechuanaland. I also found that the village political structure, far from being the authoritarian chiefship that the South Africans liked to imagine, was in fact an extremely anarchic, democratic, badly organized but open society in which lots of criticism and deviance was tolerated.

CF: That is why you called the book *An African Democracy* [1970]?

AK: Yes, and I was criticized for this because women did not have full political rights and there was a small minority of Bushmen servants/serfs, who of course were

excluded from the system. All that was true, but still, for the men and, indeed, for the majority of people in the village it was an extremely open and democratic system. Besides, I came back to Bechuanaland partly because in 1966 it had become independent—it had become Botswana—and I wanted to see what changes and consequences this had at the local level. And so my book is also *An African Democracy* in the sense of Botswana's becoming a democratic state and the village's becoming a local-government segment of an independent democratic state.

FN: How did you begin to work on the history of British anthropology?

AK: It started as a complete accident. When I went back to England in 1970 I reformed my friendship with Schapera, who was by then already retired, and he was editing a series of introductory texts in anthropology for Penguin Books. I don't know why he invited me to write a book in the series on the history of British anthropology. It was a completely ridiculous idea. I was 30 years old and inexperienced, and he was asking me to write on this topic on which I had never done any research or publication or even thought of. So I said yes, mostly because I needed the money, and I wrote this book, working nights, reading everything I could find. There were no archives at that stage, because the Malinowski collection, which was the main archive, was being formed by [Raymond] Firth, and he denied me access to it because it wasn't yet open. He refused an interview with me, but some of the others gave me interviews—almost entirely lies and fantasies and propaganda, except for Lucy Mair, who was completely honest with me. But there were enough published texts around for me to make some sort of account. The book [1973] came out while I was in the field in Jamaica, and suddenly people started sending me these hysterical reviews. The *Times Literary Supplement*, which in those days had anonymous reviewers, had an anonymous review of three pages—three pages of abuse. I was sitting there in Jamaica getting these things, and I didn't know what was going on in England. It was horrifying, but at the same time the book was selling very well.

CF: Both groups—the structural-functionalists and the more Malinowski-oriented ones—had the same kind of reaction?

AK: Everyone was completely upset. You must remember that this was still a very hierarchical and conservative Britain. The number of professors of anthropology was still very small—there were maybe 60 or 70 anthropologists. So everybody knew each other. And the professors were very big, big men, barons; Max Gluckman, Firth, E. E. Evans-Pritchard, Meyer Fortes, these were big, big guns, big characters, very important and self-important. And all of them thought that when they retired they were going to write a history of British anthropology, explaining anthropology, explaining how it all led

to them (or at least to their school). And suddenly, here was this nobody, this youngster—what right had he got to write this history of anthropology? And also to criticize them? Some were upset simply because I gave more space to so-and-so than to them or left out their most important contribution. In any case, they all hated it, and it shows how ignorant I was that I did not even know that it was going to cause this hysterical reaction.

CF: Even Meyer Fortes reacted in that way?

AK: No. It's different, of course, when you know somebody. But the others were horrified. They wrote me a series of extremely critical and abusive letters. It was an astounding reaction.

CF: And Mary Douglas, who was at the University College at the time?

AK: She is a friend of mine, and at the University College something else was happening. It was just around this time, when I came back from Jamaica in 1974, that a Marxist cargo cult suddenly developed in anthropology at the university. It was being argued that all of the old books had to be burned, and maybe the old professors as well, and then a new world would arise with a new kind of anthropology in which everyone would be equal and free and the empires would collapse. This was very exciting; young anthropology professors and students were sitting there at lunch-time reading Althusser and all those texts. It was like Bible sessions—all collective sessions, and everybody had to belong. And there were only two people in the department who were completely unimpressed by all of this, Mary Douglas and me. And we were put under a lot of pressure by the students and these young activist professors. So we supported each other and had a certain kind of solidarity.

FN: How do you see, in theoretical terms, the relationship between your work on the history of anthropology and your empirical research based on fieldwork?

AK: I'm not at all sure that there is a close relationship or a necessary relationship rather than an accidental biographical one, but the fact that I do both kinds of research obviously means that they influence each other. For example, I wrote a book called *Wives for Cattle* [1982], which was a comparative study of southern African traditional kinship systems. One of the things which puzzled me was the problem of lineage theory. I found that it was empirically useless in this context, and I decided to just say, "In South Africa lineage theory is of no use." But at the same time—and this again was another accident—I was asked by the *Annual Review of Anthropology* to write an article on lineage theory (and, again, I'm not sure why Meyer Fortes recommended me), and I decided to look at it historically. And looking at it historically was a way of answering the question why this theory was not useful and why it had been so influential. Where did it come from? What connections did

it make? And this article then led to a book called *The Invention of Primitive Society* [1988]. So I suppose a normal anthropologist would be writing about, say, comparative ethnography, coming up with the problem of lineage theory and maybe writing a theoretical critique of lineage theory. My own particular intellectual peculiarity is that when I begin to think about these theoretical questions I tend to formulate them partly in historical terms. This happened to me again now, with this problem of "culture." I did not want to write a historical study, but I was troubled with the question of culture, which seemed to me to be very influential at the moment in various anthropological debates in the United States and also in Britain. It seemed to me to be unacceptable, for various empirical and political reasons. Given my background in South Africa, I did not like this kind of theory, and to try and work out what the theory was about and what was right and wrong about it, for me the obvious way was to situate it historically. For me, theoretical questions demand some kind of historical treatment, and these theoretical questions arise because I am concerned with the answers that are being given to ethnographic questions or comparative anthropological questions. I don't see myself as a historian. I am more someone who thinks historically about theoretical questions in anthropology.

FN: Let us come back to fieldwork. How did Jamaica happen?

AK: You see, everything that I'm telling you is a series of accidents.

FN: This may be the case for all of us, but the issue is how to turn the accidents into problems and questions.

AK: That's fine, but you mustn't ask me to rationalize it retrospectively as a series of logically coherent decisions. My wife did her Ph.D. fieldwork on the Goan community in East Africa, so we became very interested in these Goans and applied for a grant to go together and do fieldwork in Goa. We got the grant, but the Indian government, at that moment, was involved in a war with Pakistan, and there were problems in Goa, and they were very reluctant to let us come. M. G. Smith, who was my head of department, was in fact a Jamaican, and a new government, a slightly radical new government, had just been elected in Jamaica under Michael Manley. Smith was an old friend of Manley's, and Manley had asked him to take over a lot of the social research. So he said to me, "Look, the Indians are not going to let you come; it's quite clear you're going to lose your sabbatical. I am going to employ you to do research in Jamaica." As I always say, like Christopher Columbus I aimed at India and ended up in Jamaica. But I knew nothing about the Caribbean. We had done all our preparation to do research in Goa. I read a few things and went out and did a year's fieldwork in different parts of Jamaica, and I wrote a book about it criticizing the established Caribbean anthropology, which was expressed in terms of a

plural society made up of different colour and class sections, and so on [1976]. It seemed to me a completely false picture of what I was seeing empirically. I prophesied, but not strongly enough, that Jamaica would remain a two-party democracy. People were saying that there was going to be a coup and it was going to become a one-party state. It seemed to me that, in fact, if you looked at the structure of the parties and the depth of their support, it was quite clear that the country was very evenly divided, at all levels, between these two parties, which were very strongly institutionalized in the villages.

CF: I want to know how you organized your fieldwork in Jamaica. It seems to be a different matter from studying a Kalahari village because, as you point out, you were studying Jamaica as a country. Of course, it's a little island, but . . .

AK: It is a little island which then had two million people. I had a choice between being attached to the university social science faculty or to a national planning agency which was in the prime minister's office. I decided to be in the agency because it would give me better access to all that I wanted to study. They also promised me—and then delivered—complete editorial freedom. And I said, "What are you interested in?" And they had a very general question: In Jamaica about one-quarter of the population lived in Kingston, which was the main city and which had very high unemployment and very bad slums. But people kept pouring in from the countryside, whereas the big agricultural employers in the countryside were complaining of a shortage of labour. So the question was, What is happening here? Is there any way we can transform the situation and stabilize the migration? In order to answer this question, I worked for a few months in a squatter area in Kingston (a sort of a slum), a few months in a small country market village near a country market town, right in the mountains, quite far away from Kingston, a few months on the coast, in a tourist area, and a month on a sugar estate, attempting to get the different kinds of data to answer this question. Also, I had the government statistical data on employment and on agriculture. And I came up with an analysis in terms of landholding patterns, land inheritance patterns, and employment patterns on the sugar estates and plantations and among rural migrants and Kingston-born people in Kingston. It turned out that the rural-born people, although they were earning very little, still had jobs in Kingston at one time or another, but they were taking jobs that the Kingston-born people, who were in the "black" economy, were not prepared to take. So once you understood the mechanisms of the situation, the pattern was quite rational. But in doing this I discovered other sorts of things. The one that interested me most was right at the start when we moved out of Kingston into the country. We were living in a small village, and I started walking round the village, going into the rum shops, and nobody would talk to me—nobody except one upper-class woman. It was very depressing. I

stayed there for about a week and then one Sunday I walked up the hill into the next village, and as I walked into the village everyone started talking to me, going absolutely hysterical—they'd come and visit me, come to my house, come and have a drink and have dinner. Finally they explained to me what the situation was. Since they knew I came from the government, from the national planning agency, they assumed that I was a supporter of Manley and a member of the People's National party. The first village I had gone to was almost solidly the opposition Jamaica Labour party, but as soon as I walked over the hill I came into a village which was a government village, so I was one of their people. I then found that the whole countryside was a patchwork of villages. One would be PLP and the next one JLP, PLP, JLP, PLP, JLP, and so forth, because if your rival village was one party, you were the other party. In Kingston the slums were the same—one was this party, the other was the next party, and so on. It ran with an extraordinary structure.

FN: After Jamaica you went to Holland?

AK: Yes, about two years later. There was a large anthropology department there, mostly specializing in Indonesia. There was also some tradition of African anthropology there, and so I was asked to come and build this up a bit more. And that was where I wrote *Wives for Cattle*; I was working largely on historical South African material. I also did some very brief fieldwork with some students in Mauritius, and I became a Dutch professor. It was a nice small-town life. And then I was invited to spend a year at Stanford at the Center for Advanced Studies in Behavioral Sciences, which was a wonderful year, 1981. I met all sorts of people, and I realized that I was becoming provincial, and my wife decided that we should go back to London.

CF: *Wives for Cattle* is the last book in which you worked directly with South African materials?

AK: Since then I have published a number of articles, mainly historical articles on precolonial South Africa—first in *South Africa and the Anthropologist* [1987] and now, together with some others, in a collection of essays called *Among the Anthropologists* [1999a]. I think that it's not so much that I am moving away from South Africa as that I've been moving more from ethnographic to more theoretical and historical kinds of studies—although I am starting a comparative project with some friends in Europe on family businesses.

FN: What is that? I am curious.

AK: The paradox is this: People imagine a modern market capitalist society with businesses that are extremely rational, but the reality is that if you look at most European societies, between 85 and 95% of all businesses are family businesses. Now, that is very unexpected, given the whole theory of what the market is and how

capitalism works. And these aren't just small businesses—Fiat, Olivetti, in England some of the major banks, huge companies, and so on. Even in the United States over 80% of all businesses are family businesses. These family businesses make you think in a different way about capitalism. If you look at the literature on family businesses, it's almost entirely produced by the business schools. Their experts have developed case studies of these businesses and certain problems they face—succession, training of the new generation, raising capital. In other words, the family businesses are presented as a series of problems because they don't conform to some abstract capitalist idea. And so the advice that is always given to family businesses is to become more like ordinary companies. But, if you ask an anthropological series of questions, then you begin to ask, why is it still the case that over 85% of businesses are family businesses? If you take a Darwinian view, they are more successful, they survive, so they must be doing something which is better than other kinds of business. And what are the Darwinian advantages that they have? Obviously, it is some kind of trust, a trust which is built on the ethic of kinship and on having a different kind of economy within the company from the economy outside. The economy within the company is an economy of mutual services, of trust. So it is a series of family relationships, and you can ask whether it is a capitalist economy, a gift economy, or a kinship economy not at the periphery but at the heart of the capitalist system. Then this makes you think about kinship in a different way, because if you have a family business then it is going to affect family, marriage, all these kinds of things within the family.

FN: How did you begin to think about the book on culture [1999b]? It is a book centred on American anthropology. Can you speak about your relationship with American anthropology?

AK: It was only when the postmodernist movement got going that I had a feeling that there was a major movement in American anthropology that I regarded as extremely dangerous for the development of anthropology in the States and internationally—extremely destructive of what seemed to me to be the important kinds of anthropology, which were serious, empirical, comparative, addressing major issues of theoretical importance and public concern. All this was being frittered away by a kind of rather superficial extreme relativism, a very adolescent kind. And this was, if you like, another cargo cult, after the Marxist cargo cult, but much worse, much more destructive. It became more and more powerful in Europe; in England many of the major anthropologists were completely captivated by it. Anyone who was doing any other kind of anthropology was viewed as standing in the way of progress. So I began to look for allies, people who like me were disenchanted with this and had the feeling that a sociological, comparative anthropology, a more positivist anthropology, had to be sustained. I found them all over Europe, and this was one of the impulses

for the European Association of Social Anthropologists. That gave some people in America the impression that it was some kind of anti-American movement, which is not true. I was against this particular movement in American anthropology, which is a different kettle of fish.

FN: When you went to the Institute for Advanced Studies at Princeton, in 1994–95, you already had the project for the book?

AK: I went to Princeton with the idea. Geertz invited me, and I went. I wasn't sure that it was viable. What I wanted to do was a historical critique of the development and uses of this idea of "culture" in American anthropology but also more broadly. And I had been there four or five months and I felt, "It's impossible, I can't put it together." And then I suddenly saw a structure for the first part of the argument. And once the structure was in my mind—I had done all this reading and I had all of these notes—I just sat down every morning and wrote. And at the end of ten days I had about 100 pages. It was a strange experience for me. I had never had this experience of a book's suddenly emerging in this way.

CF: It was as if you were writing the American side of *Anthropology and Anthropologists* [1973].

AK: But this time I knew it was critical beforehand, so it was an attack. *Anthropology and Anthropologists* was not an attack on British anthropology.

FN: I am interested in the relationship, if one exists, between your experience as the editor of CURRENT ANTHROPOLOGY and the book you have just written.

AK: This again is one of those accidents. I soon discovered that CURRENT ANTHROPOLOGY has a very specific intellectual space. First, it is the only really international journal of anthropology. Although it is obviously dominated by the Americans, it has always had a policy of trying to get as international a list of contributors and readership as possible. Secondly, it was the only major anthropology journal that was still structured on the four-fields model and an evolutionist kind of model. I gradually became very interested in these evolutionist questions, which had formed no part of my intellectual background of education and of British-European anthropology. This was the time when some very interesting developments were occurring in Neanderthal, European studies, and various other kinds of evolutionary studies, and I was meeting some of the most interesting people in the field, and I was excited about what they were doing. So I wrote a sort of general, popular book on the subject, a kind of Darwinian anthropology [1994]. The intellectual part of being editor of CURRENT ANTHROPOLOGY was working out this stuff for this book. The other thing that I learned from that experience was that for the first time I was really put in touch with world anthropology, and I began to get the idea of the future

of anthropology as something not dictated by a metropolis but potentially a truly cosmopolitan debate. And I began to become more appreciative of the kinds of innovation that were coming out elsewhere.

CF: Let me ask you a very general question which is not biographical in any sense. What is it that you like about the anthropology that is being produced now? If you were asked to pick some books that you admire, what would you say?

AK: Well, I've been reading some of the papers produced here at the [National] Museum [Rio de Janeiro]. This is a good example of the anthropology I like. It is empirical, it is involved with a social and political context, it is critically sophisticated—aware of a number of models and theoretical approaches without being dogmatic or closed about any of them. It is aware of other related work being done in the same kind of area from other traditions, and it approaches this kind of research in a respectful but critical and open-minded way. The anthropology that I don't like, for the reasons that I've gone on about ad nauseam elsewhere, is the extreme relativist and anti-empirical kind of anthropology and the ideological kind of anthropology in which the agenda is set by other kinds of ideological issues—feminism, ethnic nationalism, or whatever. The kind of anthropology I like I would call rational, humane, sophisticated, cosmopolitan.

FN: You said when you were talking about moving to England that you wanted to make your life in Africa. What do you think about that today?

AK: When the transition occurred in South Africa, in the late 1980s, I was offered a couple of very nice positions in South Africa at some of the leading universities, and I desperately wanted to go. My wife absolutely refused, because, she said, "Our children have grown up in Europe. They're not going to come there; they'll want to make their lives here in England. I don't want to be living over there with my children, perhaps one day grandchildren, here in Europe. They are here, this is where we've lived, and this is where we'll stay. So no." And I was bitterly disappointed, but of course I could see the logic of the argument, and I could understand and appreciate it. It was true, and it was only at that moment that I appreciated that I had been living, like many immigrants, with this fantasy of return, and that when at last the opportunity came that I could have returned, it was too late. I couldn't. And that was a very difficult moment for me, really, because I had always had this image of myself as being a South African, not as being English. And I had to realize, as many immigrants do, the reality that I had become, of course, something different.

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Semes and Genes in Africa¹

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This report has two general aims: to explain the distribution of cultural practices and beliefs across the landscape in Africa and to demonstrate how genetic, linguistic, and geographic information can be used to understand the nature of culture. We focus on ethnic groups that share cultural units (schemas or practices) and utilize genetic, linguistic, and geographic data to evaluate the processes that help to explain this sharing. Following Cavalli-Sforza, we call these units “semes” rather than “memes” (Dawkins 1976, Durham 1991,

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Boyd and Richerson 1985) because “seme” comes from “sign” and emphasizes the symbolic nature of culture. Our approach is sometimes called “coevolutionary” or “dual-inheritance” because it identifies relationships between genes and culture. We prefer to call it evolutionary cultural anthropology (Hewlett and Lamb n.d.) because the major theoretical contributions to date (Cavalli-Sforza and Feldman 1981, Boyd and Richerson 1985, Durham 1991) emphasize understanding the evolutionary mechanisms and properties of culture.

Studies of the relationships between biology and culture are not looked upon favorably in anthropology, partly because they tend to be linked with discussions of race and culture. Franz Boas countered racism in the early 1900s by demonstrating that “races” had no inherently different biologies and cultures. He saw the processes as unconnected because biological mechanisms were so slow to change while culture could change very rapidly. Boas was interested in explaining cultural diversity and rejected the notion that race or biological differences could help explain this diversity. Discussions of race and culture were major components of early anthropology textbooks (Boas 1938, Kroeber 1923), and combating racism and ethnocentrism continues to be important in most anthropology courses. We suggest that cultural mechanisms can help to explain why genes and culture may coincide. This does *not* mean that the biology determines culture; indeed, culture often determines genes, as in the case of adult lactose absorption (Cavalli-Sforza and Feldman 1976, Durham 1991). Our work is consistent with a Boasian perspective in that we are interested in trying to understand how particular culture histories can help explain cultural diversity in Africa, but in contrast to Boas we use genes and language as tools for interpreting that diversity. This study extends Boas's and Kroeber's work by identifying specific cultural mechanisms and models which help to explain cultural diversity and interpret cultural histories.

EXPLANATORY MODELS

Why do cultures share semes? Three broad explanatory models are usually offered: (1) cultural diffusion, borrowing or diffusion of the seme from neighbors, (2) local adaptations, in which individuals develop similar semes to adapt to similar natural and social environments, and (3) demic diffusion, the movement of peoples and their semes to new areas. The first model is a trademark of the Boasian and Kroeberian cultural relativist tradition in anthropology. This tradition deemphasizes the adaptive nature of culture (i.e., the impact of natural and social ecologies) and suggests that semes arise primarily from human imagination and mindful play and may take their own courses. The second model is common in cultural anthropology and has its roots in Julian Steward's (1955) cultural ecology. In the 1970s, cultural ecology was modified and called “cultural materialism” by Marvin Harris, and in the 1980s and 1990s it was modified further and reemerged as “evolutionary ecology.” These approaches consider most semes adaptive (i.e., enhancing

the Darwinian fitness of individuals or groups). The third model is characteristic of studies such as those of Cavalli-Sforza, Menozzi, and Piazza (1993), where it describes the repeated expansions of a group generated by the development of an innovative technology or type of social organization. The innovation leads to population growth, migration, and intermarriage with those without the innovation (i.e., gene flow in the direction of those with the innovation). Genes, semes, and language move with the innovation. The semes that move with people may or may not be adaptive; many are likely to be neutral. Some semes may be adaptive in some environments or circumstances and neutral or nonadaptive in others. Semes are conserved through specific mechanisms of cultural transmission. The demic-diffusion model is seldom used by cultural anthropologists. Cultural diffusion may be popular among postmodern and ethnohistorical anthropologists because it is so easy to see in today's world.

MECHANISMS OF CULTURAL TRANSMISSION

Four mechanisms of transmission underlie these models (table 1). The first, vertical transmission, is most similar to genetic transmission. Mathematical analysis has shown that semes transmitted in this way are highly conserved (Cavalli-Sforza and Feldman 1981). Vertical transmission is especially pronounced in infancy and early childhood, in part because of parent-child proximity and attachment. The second, called the group effect by Cavalli-Sforza and Feldman (1973) and frequency-dependent bias by Boyd and Richerson (1985), is the process whereby individuals acquire semes that occur frequently in the population on the assumption that they are likely to be adaptive. This mechanism also tends to

maintain the status quo. Henrich and Boyd (1998) argue that vertical transmission alone is not sufficient to explain group-level conservation of semes and that the group effect increases the frequency of a seme beyond what is expected from vertical transmission. Vertical transmission and the group effect are the mechanisms by which semes are conserved in demic diffusion.

The third mechanism, horizontal transmission, is based upon epidemiological models of disease transmission. As the frequency of interaction with or exposure to an unrelated individual with a seme or disease increases, it becomes more likely that one will adopt a seme or catch a disease. As the frequency of interactions between individuals of different communities increases, it becomes more likely that they will adopt aspects of each other's culture. Horizontal transmission may be (1) between generations (called oblique), (2) within a generation (the origin of the term "horizontal"), or (3) one-to-many (characteristic of highly stratified urban industrial societies in which teachers, leaders, TV, and the Internet transmit information). Culture change with horizontal transmission can be rapid; the one-to-many form is especially conducive to rapid culture change and, though common today, was rare in the past. Horizontal transmission is the prime mechanism of cultural diffusion.

Trial and error is a process that contributes to local innovation and adaptation. Individuals observe or hear about alternative semes and critically evaluate their advantages and disadvantages. This evaluation may lead to a synthesis of existing semes (i.e., recombination) or the development of an entirely new seme. The innovative seme is, at first, often transmitted horizontally. The trial-and-error process takes place from infancy on, but a sub-

TABLE 1
Three Explanatory Models for the Sharing of Semes across Cultures

Model	Mechanism	Features	Age Most Pronounced	Rate of Culture Change	Favoring Environmental Conditions
Demic diffusion	Vertical	Similar to genetic transmission; parent-to-child; preserves status quo	Infancy and early childhood	Slow	Stable
	Group effect	Frequency of seme in population impacts acquisition; preserves status quo	Late childhood and adolescence	Slow	Stable
Cultural diffusion	Horizontal	Frequency of interaction impacts acquisition; epidemiological model; route of innovation	Early childhood (between generations); late childhood and adolescence (within generations)	Can be rapid	Rapidly changing
Local adaptation	Trial and error	Evaluation of alternatives; cost-benefit; source of innovation; leads to convergence	Adolescence	Slow	Rapidly changing

stantial synthetic or innovative seme that is transmitted to others is more likely to emerge in adolescence or early adulthood. The use of trial and error by peoples in similar but distant natural and social environments may lead to the development of similar semes, a process similar to evolutionary convergence.

The stability of the environment influences the utility of these mechanisms. When environments change very slowly, adaptive knowledge can be obtained at the level of vertical transmission because only modest updating of knowledge is needed. It may, in fact, be difficult to distinguish between genetic and vertical cultural transmission mechanisms that are responsible for a particular behavior, since both are vertical and conservative. This has probably led some researchers to attribute genetic causes to human behaviors in error. In contrast, where environmental change is very rapid, individuals should favor horizontal (within-generation) transmission and trial and error. In such environments, genetic systems will change too slowly to cope, and information from the parental generation is likely to be outdated and error-prone.

PATTERNS OF GENETIC, LINGUISTIC, AND GEOGRAPHIC DATA

Eight patterns of genetic, linguistic, and geographic data that emerge from the three explanatory models are shown in table 2. Demic diffusion assumes that cultures share semes because they have a common past, and therefore genetic and/or linguistic similarities are predicted. To control for the effects of cultural diffusion, the cultures should also be geographically distant from each other. Cultures are likely to share semes because of demic diffusion and associated mechanisms of cultural transmission (vertical and group effect) when they exhibit the first three patterns of genetic, linguistic and geographic data. With these patterns, cultures that share semes are far apart and share genes and/or language. Cultural diffusion assumes that cultures share semes because they regularly interact with each other, so cultures that are geographically close to one another are expected to share more semes. In order to control for other factors, the cultures should not share a language or many genes. The fourth, fifth, and sixth patterns are most likely to demonstrate cultural diffusion and horizontal transmission. Pattern 6 is the best measure of cultural diffusion, while 4 and 5 are potentially confounded by factors of demic diffusion. The last pattern indicates that cultural similarities could be explained by any of the three models, while the next-to-last is the best for predicting local adaptations.

DISTANCE MEASURES

Our study required cultural (seme), genetic, linguistic, and geographic data on the same ethnic groups, and the primary limiting factor in selecting a sample was the availability of genetic data. The *Ethnographic Atlas* (Murdock 1967, Gray 1999) provides cultural data on

over a thousand ethnic groups; Ruhlen (1991) and Grimes (1978) provide linguistic classification data on most of the world's languages, and it is easy to determine geographic distances between any two ethnic groups. To calculate genetic distances we decided to use autosomal genetic markers rather than DNA markers because only a few African populations have been examined for the latter. (For instance, the most recent study of African mtDNA genetic distances was based upon 20 individuals from 13 ethnic groups [Ingman et al. 2000], and the most recent study of African Y-chromosome genetic distances was based upon 13 individuals from 8 ethnic groups [Underhill et al. 2000].) From the genetic database maintained at Stanford University we were able to identify genetic data on 42 ethnic groups. [Additional information on methods appears in the electronic edition of this issue on the journal's web page]. Six cases had to be eliminated because there were no corresponding cultural data in the *Ethnographic Atlas*, and the remaining 36 cultures became the basis for all comparisons. The 36 ethnic groups had data on 13.97 loci on average (range 7–26) and 34.5 independent alleles (range 16–74). Genetic distances between pairs of ethnic groups were based upon an average of 22.7 independent alleles (range 14–70). Nei and Roychoudhury's (1972) method was utilized to calculate the genetic distance for each pair.

Cultural distances between pairs of societies were calculated with a method similar to Driver and Kroeber's (1932) "G" statistic. Each of the 630 pairs was compared for similarities and differences with regard to the 42 traits (e.g., mode of marriage) in the *Ethnographic Atlas*. Each trait had several alternative semes, and each culture was coded for one seme in each category. If either of the cultures had missing data for a trait, no comparison was made on that trait. A total of 109 semes were compared for each pair of societies.

Since no Swadesh word list was available for most of the cultures and no other method for measuring linguistic distance exists (see Chen, Sokal, and Ruhlen 1995), we developed a method somewhat similar to that described above for cultural distances. From Ruhlen's (1991) classification of languages we determined the number of linguistic categories in which two languages were similar and different.

Geographic distances between two cultures were calculated using the haversine formula (Sinnott 1984), which uses spherical trigonometry to calculate great-circle arcs. This "as the crow flies" measure is limited in that it does not take into account physical features such as mountain ranges, rivers, swamps, and other things that may help or hinder the movement of peoples or semes.

This paper also utilizes another measure, called the "clustering index" (Guglielmino et al. 1995), to evaluate the opportunities that members of one culture might have of acquiring the seme in question from neighboring cultures. It assesses the density of and geographic proximity to other cultures with the same seme.

TABLE 2
Patterns of Data Generated by the Models

Model	Data Pattern			
	Genetic	Linguistic	Geographic	Features
Demic diffusion	Similar	Similar	Distant	Peoples with same language and semes moved some distance from each other
	Similar	Similar	Distant	Peoples intermarried or shared semes in the past but either spoke different languages then or adopted a new language when they moved some distance from each other
	Different	Similar	Distant	No or limited intermarriage in the past, but peoples spoke similar languages and shared semes; one may have moved to a new area and intermarried with new neighbors but retained language and semes
Cultural diffusion	Similar	Different	Close	Intermarriage between peoples that speak different languages but have acquired seme from common neighbors or each other
	Different	Similar	Close	No or limited intermarriage but peoples share language and semes with common neighbors or each other
	Different	Different	Close	No or limited intermarriage and different languages but peoples have acquired semes from common neighbors or each other
Local adaptation	Different	Different	Distant	–
Multiple confounds	Similar	Similar	Close	–

RESULTS

Trees. The genetic tree for the 36 populations demonstrates that genetic distances between most African populations are relatively low by comparison with similar trees in the Americas because of the frequent intermarriage between ethnic groups noted by several ethnographers (e.g., Goody 1976) and the relatively recent Bantu expansion. The linguistic tree has four distinct branches consistent with the four linguistic phyla in Africa. This was expected, given our use of Ruhlen's classification

system, but it did support the usefulness of our linguistic-distance methodology. The cultural tree is of course more complex, in part because it is influenced more by horizontal transmission than the other two trees, but the branches did identify three modes of production: hunting-gathering, farming, and pastoralism. This suggests that the semes coded by Murdock are often linked to a particular mode of production in Africa.

Language, ecology, and mode of production. Table 3 lists the means and standard deviations of the four dis-

TABLE 3
Means of Distance Measures and Clustering Index

	Number of Cultural Pairs	Mean	S.D.
Genetic distance	630	0.038	0.046
Cultural distance	630	0.600	0.124
Linguistic distance	630	0.879	0.239
Geographic distance (km)	630	2,988	1,681
Clustering index (mean of 109 semes)		0.385	0.195

tance measures and the clustering index. As genetic, linguistic, and cultural distance increases, the similarities for those values decrease. For instance, the average genetic distance is 0.038; this means that the 36 African cultures share 96.2% of their genes for the alleles measured in this study. The average geographic distance of 29.88 means that cultures are, on average, 2,988 km apart. (The measure in degrees can be converted approximately into kilometers by multiplying by 100.) Africa is huge, and most ethnic groups in this study are far from one another. Consequently, it is unlikely that geographic distance will be a useful measure of potential cultural diffusion between cultural pairs in this study. The clustering index for a particular seme, which is based upon all African cultures in the *Ethnographic Atlas*, will therefore be utilized to evaluate the opportunity for cultural diffusion.

The linguistic distance of 0.88 indicates that two African languages in this sample have, on average, 12% of linguistic features in common. While our distance method seems appropriate for evaluating general relative

differences in languages, it exaggerates the differences, in particular, those between languages of different phyla, which are assigned a distance of 1.0 and a similarity score of 0. Most of the distance scores were based upon languages from different phyla, and this contributed to relatively high linguistic distances. If Swadesh or similar word lists were available for each language, the measure would be more precise. Even languages from different phyla are likely to have some cognates.

Tables 4–6 summarize the four distance measures for linguistic phyla, natural environments, and modes of subsistence for the 36 ethnic groups. (The clustering index is not listed because it is not linked to a specific ethnic group.)

Table 4 shows that the Niger-Kordofanian ethnic groups are near the mean for all measures but tend to be closer genetically than the ethnic groups in the other phyla. The Afro-Asiatic ethnic groups are slightly more heterogeneous in genes and culture than ethnic groups in the other three phyla. Khoisan-speakers have more in common culturally than do Niger-Kordofanian- and Afro-Asiatic-speakers. The Nilo-Saharan cultures in this study are closer genetically and culturally than those of other linguistic groups. While it is possible that this is because they are somewhat closer geographically, on average, it is more likely to be a result of the relatively recent emergence of this group. The fact that the Sahara is a relatively recent desert and the genes, languages, and cultures in this group show low variance makes this especially likely.

Table 5 suggests that ecology has little influence on any of these measures. Cultural ecologists might predict that cultures that share similar natural ecologies should be culturally similar, but no pattern emerges from these admittedly limited data. Table 6 examines the three

TABLE 4
Genetic, Geographic, and Cultural Distances for Cultures in the Same Language Phylum

	Niger-Kordofanian			Khoisan			Nilo-Saharan			Afro-Asiatic		
	<i>n</i>	Mean	S.D.	<i>n</i>	Mean	S.D.	<i>n</i>	Mean	S.D.	<i>n</i>	Mean	S.D.
Genetic distance	18	0.020	0.015	4	0.032	0.018	7	0.013	0.006	7	0.054	0.045
Geographic distance (km)	18	3,210	1,837	4	1,940	1,181	7	1,218	610	7	2,881	1,472
Cultural distance	18	0.587	0.135	4	0.483	0.140	7	0.456	0.121	7	0.684	0.087

TABLE 5
Genetic, Geographic, Cultural, and Linguistic Distances for Cultures in Similar Natural Environments

	Sahel Semidesert			Wet Savannah			Tropical Forest		
	<i>n</i>	Mean	S.D.	<i>n</i>	Mean	S.D.	<i>n</i>	Mean	S.D.
Genetic distance	12	0.039	0.039	18	0.037	0.0551	6	0.023	0.016
Geographic distance	12	3,734	1,795	18	2,889	1,734	6	2,431	1,214
Cultural distance	12	0.591	0.118	18	0.540	0.126	6	0.663	0.118
Linguistic distance	12	0.891	0.298	18	0.805	0.325	6	0.791	0.282

TABLE 6
Genetic, Geographic, Cultural, and Linguistic Distances for Three Modes of Production

	Hunter-Gatherers			Farmers			Pastoralists		
	<i>n</i>	Mean	S.D.	<i>n</i>	Mean	S.D.	<i>n</i>	Mean	S.D.
Genetic distance	3	0.036	0.015	23	0.029	0.044	10	0.047	0.045
Geographic distance	3	1,939	871	23	3,267	1,705	10	3,341	1,618
Cultural distance	3	0.297	0.067	23	0.562	0.115	10	0.569	0.103
Linguistic distance	3	0.889	0.192	23	0.793	0.296	10	0.914	0.212

modes of production among the groups. The only pattern to emerge is that the three hunter-gatherer groups (San, Mbuti, and Hadza) are the most likely to share many elements of culture.

Explanatory models. The determination of which seme fit a particular explanatory model was based upon *z*-scores for genetic distance, linguistic distance, and the clustering index. The *z*-scores provide a measure of how different the mean score for a particular seme is from the overall mean and also indicate the direction of the difference. A negative *z*-score indicates that the cultures are genetically or linguistically close, while a positive *z*-score indicates that they are distant in relation to the overall mean distance. A negative *z*-score for the clustering index means that the cultures that share the seme are relatively distant from each other, while a positive *z*-score means that they are more clustered. The criteria for determining the fit between a seme and an explanatory model were as follows: (1) If two or three *z*-scores were below 0.5, no fit could be determined; (2) if two of the three *z*-scores were above 0.5 it was considered a trend; (3) if one of the *z*-scores was greater than 1.0 and another greater than 0.5 it was considered a pattern; and (4) if two *z*-scores were above 1.0 and the third above 0.6 it was considered a strong pattern. A *z*-score of 0.5 was selected as the minimum because about 70% of the other seme averages would be below or above the overall mean.

It was not possible to discern an explanatory model for 35 semes (32%), and 29 of the semes (27%) had two potential explanatory models because one of the distance measures or the clustering index did not reach 0.5. We were able to identify a primary explanatory model for 45 (41%) of the semes in this study. Given the complex nature of cultural processes, it is not surprising that semes are influenced by several mechanisms and models.

Table 7 lists the semes that fit a particular model, and table 8 summarizes the data by seme category. The demic-diffusion model explained the greatest number of semes (20) and was especially important for explaining kinship, family, and community semes. The data are consistent with the results of recent studies (Guglielmino et al. 1995, Pocklington 1996, Burton et al. 1996, and Jones 1999) indicating that kinship and social organization in Africa and other culture areas reflect the expansion of groups with particular kinds of kinship and social organization. The semes explained by demic diffusion and represented by the greatest number of cultures

are often thought of as classic features of sub-Saharan African social structure: independent polygynous families with wives in separate dwellings, no marriage with first or second cousins, clan-based neighborhoods, and shifting cultivation (i.e., horticulture). The demic-diffusion model was also particularly important for explaining political stratification above the community. The data indicate that political complexity in Africa is primarily due to expansion of particular peoples rather than cultural diffusion or local adaptation.

Cultural diffusion explained 12 semes and was especially useful for explaining the distribution of house construction and the postpartum sex taboo. Since the clustering index is relatively high for these semes, it is also possible that the availability of particular materials in a local ecology may influence the seme.

Semes that have multiple confounds are equally distributed over all six seme categories. These semes could be a later stage of demic diffusion in that groups that slowly expanded, shared semes, and continued to live in proximity to one another or groups that had always lived next to each other may have frequently intermarried and shared semes. Matrilineal clans are a good example of this pattern, as there is a well-known "matrilineal belt" across south-central Africa, but we are unable to determine from these data whether the distribution of matrilineal clans is due to the expansion of a group with matrilineal descent or to the development of this descent system by one group and its gradual incorporation into the cultural systems of neighbors, possibly in order to marry into the group.

Semes linked to local adaptation are particularly interesting because they are the aspects of culture that Steward (1955) was trying to understand with his concept of "multilineal evolution." The four semes listed appear to be variations of demically diffused semes. Particular natural and social conditions have led to the independent development of small (versus large) extended families, the democratic (versus hereditary) election of a headman, class elites based upon their control of scarce resources (versus hereditary classes), and male circumcision in late childhood (versus adolescence).

Table 9 examines the means of genetic and linguistic distances and the clustering indexes of the 45 semes that fit into only one explanatory model. As expected, they fit the patterns described in table 2. The linguistic distances between two groups do not help to distinguish

TABLE 7
Semes and Explanatory Models

Number of Cultures Sharing Seme	Description of Seme and Model	Seme Category
	<i>Demic Diffusion</i>	
3	Bride service	KIN
6	Large extended families	KIN
12	Independent polygynous families, wives with separate rooms	KIN
10	Agamous communities	KIN
11	Exogamous clan communities	KIN
15	No marriage with first or second cousins	KIN
3	Inheritance of real property: to children, sons more	KIN
3	Inheritance of movable property: patrilineal heirs over sons	KIN
5	Inheritance of movable property: to children, sons more	KIN
24	Level of community organization: clan neighborhoods	KIN
4	House making: m + f different tasks, equal participation	SEX
8	Fishing: men only	SEX
5	Fishing: both genders participate, men do more	SEX
4	House wall material: wattle or mats	HOUSE
3	Slavery	STRAT
9	Hierarchy above community, stateless	STRAT
11	Hierarchy above community, petty chiefdom	STRAT
11	Hierarchy above community, small state	STRAT
18	Shifting cultivation	SUBS
7	Premarital sex prohibited but weakly sanctioned	VAR
	<i>Cultural Diffusion</i>	
3	Inheritance of movable property: matrilineal, to sister's son	KIN
3	Level of community organization: villages	KIN
5	House ground plan: rectangular	HOUSE
12	House wall material: walls indistinguishable from roof	HOUSE
3	House roof shape: flat	HOUSE
3	House roof shape: gabled	HOUSE
3	House roof material: earth or turf	HOUSE
7	Agriculture: m + f equal participation, no task differences	SEX
3	Weaving: m + f different tasks, equal participation	STRAT
8	Intensive agriculture	SUBS
5	Postpartum sex taboo, 1-6 months	VAR
8	Postpartum sex taboo, 1-2 years	VAR
	<i>Multiple Confounds</i>	
5	Matrilineal clans	KIN
6	Duolateral cross-cousin marriage	KIN
8	Iroquois kin terms for cousins	KIN
9	House making: men only	HOUSE
4	House roof shape: beehive with pointed peak	HOUSE
5	Boat making: men only	SEX
11	Agriculture: both genders participate, women do more	SEX
6	Weaving: men only	STRAT
11	High god: not concerned with human affairs	VAR

TABLE 7
(Continued)

Number of Cultures Sharing Seme	Description of Seme and Model	Seme Category
	<i>Local Adaptation</i>	
9	Small extended families	KIN
3	Headman succession: nonhereditary, through election or consensus	KIN
3	Elite stratification; elite control scarce resources, land	STRAT
7	Circumcision, in late childhood	VAR

NOTE: KIN, kinship, family, and community; SEX, sexual division of labor; HOUSE, house construction; STRAT, social stratification; SUBS, subsistence and settlement; VAR, various.

demic from cultural diffusion; genetic distance and the clustering index are better predictors for these two models. All three measures are important for understanding and predicting the other two explanatory models.

Table 10 examines the relationships between genetic distances, linguistic distances, and the clustering indexes. Significant relationships exist between language and culture, genes and culture, and language and the clustering index. The relationship between genes and language is high but does not reach significance ($p = .09$). Cultural anthropologists often play down the relationship between language and culture because they can always point to instances in which they clearly do not go together (e.g., Bantu-speaking foragers and farmers in Central Africa have dramatically different cultures). While there are several exceptions, these admittedly limited data indicate a significant relationship between language and culture in Africa.

Cultural anthropologists argue even more strongly against a relationship between genes and culture, but again these data indicate otherwise. We hope to have made it clear why semes and genes may coincide: It is *not* because semes are hard-wired to biology but because both are affected by the conservatism of vertical transmission.

The relationship between language and the clustering index indicates that as the proximity between two cultures increases the likelihood that they speak similar languages increases. This is not surprising, because most linguistic families and branches tend to be geographically clustered. While cultural diffusion may explain some cases, demic diffusion (e.g., expansion of Bantu- or Nilotic-speaking peoples) is more likely to explain the clustering of African language families.

This study has provided limited data on the relationship between semes and natural environments. As mentioned above, the natural environment is likely to confound cultural-diffusion semes because peoples that live next to each other may share semes as well as natural environments. Ecology may also confound demic-diffusion semes because people originating in one natural en-

TABLE 8
Summary of Explanatory Models by Seme Category

Seme Category	Explanatory Models				Total
	Demic Diffusion	Cultural Diffusion	Local Adaptation	Multiple Confounds	
Kinship, family, and community	10	2	2	3	17
Sexual division of labor	3	1	0	2	6
House construction	1	5	0	2	8
Social stratification	4	1	1	1	7
Subsistence and settlement	1	1	0	0	2
Various	1	2	1	1	6
Total	20	12	4	9	45

vironment may move to a new location with a similar ecology. Elsewhere we have found (Guglielmino et al. 1995) that semes that fit the demic-diffusion model were much less likely to be influenced by ecology than semes that fit the cultural-diffusion model (6 of 20 semes under demic model, 9 of 12 semes under cultural diffusion; chi square = 6.11, 1 d.f., $p < .01$).

DISCUSSION AND CONCLUSION

In summary, the use of genetic, linguistic, and geographic data has provided a better understanding of cultural diversity in Africa. The seme analysis has indicated that (1) demic diffusion is important for understanding the distribution of semes in the categories of kinship, family, and community and political stratification; (2) cultural diffusion is particularly influential in the distribution of house construction and postpartum sex taboo semes; (3) natural and social environments appear to have led to local adaptations and development of small extended families, the democratic election of a headman, class elites based upon their control of scarce resources, and male circumcision in late childhood; and (4) significant relationships exist between language and culture, genes and culture, and language and the clustering index.

We have been cautious in interpreting these data because our sample is small. In particular, we have limited

interpretation of specific semes because of the relatively small numbers of cultures involved. The size and quality of the genetic database is improving, and we hope to conduct more precise studies in the future. Murdock's cross-cultural data have been questioned, but some have recently taken the time to check, extend, and improve upon this database (Gray 1999). It would be preferable to utilize emically defined semes, such as myths or beliefs regarding sorcery, and to conduct the study in the field rather than relying on the codes of others, but field studies of this type have not been conducted. Also, semes are not always encoded in language; some semes are experienced directly in social interactions and daily activities. Consequently, it may be necessary to define some semes etically.

Kinship and family semes are very conservative by comparison with other semes, and their distribution in Africa (Guglielmino et al. 1995, Pocklington 1996) and other parts of the world (Burton et al. 1996, Jones 1999) appears to be primarily the result of demic diffusion. The conservation is due, in part, to both vertical transmission and group effect. These semes are often transmitted and acquired at an early age and become "market traits" (e.g., ethnic clothing styles [Boyd and Richerson 1985]) that help an individual distinguish in- and out-groups. Demic diffusion and the associated mechanisms of transmission call into question the anthropological effort to demon-

TABLE 9
Mean Genetic Distance, Linguistic Distance, and Clustering Index of Semes That Fit One Explanatory Model

Explanatory Model	<i>n</i>	Genetic Distance	Linguistic Distance	Clustering Index
Demic diffusion	20	2.0 (close)	83.7 (distant)	174.2 (low)
Cultural diffusion	12	4.4 (distant)	84.7 (distant)	610.7 (high)
Multiple confounds	9	1.5 (close)	59.8 (close)	495.9 (high)
Local adaptation	4	7.0 (distant)	93.7 (distant)	218.2 (low)

TABLE 10
Relationship between Distance Measures and Clustering Index for 109 Semes

	Genetic Distance	Linguistic Distance	Cultural Distance
Genetic distance	—	—	—
Linguistic distance	0.165	—	—
Cultural distance	0.184 ^a	0.442 ^c	—
Clustering index	-0.023	-0.252 ^b	-0.013

^a $p < 0.05$.

^b $p < 0.01$.

^c $p < 0.01$.

strate that many of these semes are adaptive or functional in a particular ecology. While more precise studies of the relationships between semes and ecology are needed, this and our previous study indicate that the impact of ecology is limited.

Cultural diffusion and horizontal transmission are of tremendous importance in today's global economy, in part because of new technologies that allow rapid dissemination of semes. Most ethnic groups in this study did not have these technologies at the time they were described; consequently, this study suggests a more limited role for cultural diffusion.

Semes usually do not evolve as discrete units; they often evolve as part of a culture complex or culture core. The models, mechanisms, and methods described in this paper can help evaluate the culture cores or complexes proposed for Africa (e.g., Vansina 1990, Goody 1976), but this would take considerably more analysis. Archaeologists have argued that functional features of artifacts are adaptive whereas stylistic features are neutral and more appropriate for evaluating cultural evolution. What is stylistic and what is functional is not always clear, and the methods used here may be helpful for making this distinction in a particular region. We hope to have provided the theoretical, conceptual, and methodological tools that will allow others to examine the relationships between language, culture, and genes in any region of the world.

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The Institutional Maintenance of Celibacy¹

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From a Darwinian perspective, celibacy is a problem worthy of particular attention. After all, reproduction is

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the “key phenomenon of evolution” (Vogel 1989:xi), and “all living organisms have evolved to seek and use resources to enhance their reproductive success” (Low 2000:xiii–xiv). Celibacy, therefore, should not exist except where suitable mates are unavailable, where an individual suffers from genetic, behavioral, or psychological incapacities, or where it confers benefits to the survival or reproduction of an individual’s close genetic relatives. That this practice should occur as a conscious choice made by normal, healthy individuals in non-familial settings appears to run counter to Darwinian expectations. Yet clearly celibacy, or lifelong sexual abstinence, has been vowed and practiced in a variety of nonkin, institutional contexts for thousands of years. While Christian, Buddhist, and Hindu monastic orders are the most familiar examples, Vestal Virgins, Jains, Essenes, Protestant religious orders such as the Shakers, and even recent cults such as Heaven’s Gate have preached or practiced celibacy as well (Abbott 2000).

The rationales for individuals’ embracing celibacy are many, and the origins and functions of celibacy in particular societies are varied and complex. This report focuses on the relationship between celibacy and induced or manipulated altruism in institutional settings. Evolutionary theory predicts that individuals may sacrifice themselves for the benefit of genetic relatives, and humans generally recognize kin by means of indirect cues. These cues can be manipulated, and institutions may do so in order to maintain and reinforce celibate and other altruistic behavior in nonkin settings.

CELIBACY AND EVOLUTIONARY THEORY

There is little consensus among evolutionists regarding the patterning and implications of celibacy. Some (Betzig 1995*a, b*; Freyd and Johnson 1992) minimize its importance by pointing to its rarity. Others (e.g., Pinker 1997) suspect that celibacy is but one of many aspects of the “novel” human environment with which the mind is not designed to cope and for which adaptive explanations are pointless. Dawkins (1976:205; see also Blackmore 1999:138–39) discusses celibacy as a prime example of a meme and thus admits to “throwing out the gene” in his attempts to explain it. More generally, some suggest that a learning rule through which individuals acquire cultural traits might itself be an evolved adaptation and yet spread maladaptive traits such as institutionalized celibacy throughout a population (Boyd and Richerson 1985:205; Cavalli-Sforza and Feldman 1981; Logan and Qirko 1996).

The most rigorous attention given by Darwinians to celibacy has centered on inclusive fitness, based on kin selection theory (Hamilton 1963, 1964; Wilson 1975). Hager’s (1992:386) review of female religious claustration in medieval Europe suggests that convents benefited

nuns’ families as “depositories of excess females.” Celibacy might therefore be a familial strategy to minimize parental investment in offspring when cultural circumstances such as inheritance laws render it practical (Betzig 1995*a*, Boone 1986, Hrdy 1997). A related factor is competition for mates, as female hypergyny in stratified societies leads to an excess of both high-status women and low-status men. Thus Betzig (1995*a*:203) describes monasteries as “reservoirs of [male] reserve heirs.” Models of parental sex-ratio manipulation of offspring (e.g., Trivers and Willard 1973), extended to include human cultural manipulation, predict that families in lower socioeconomic strata should invest in daughters, while those in higher strata should invest more heavily in sons. As Dickemann (1979) suggests, celibacy, along with suicide and infanticide, appears to be patterned cross-culturally in ways that support these predictions. Additionally, Betzig (1986) has shown that male access to power is correlated with differential reproduction. Celibacy, both of women controlled by despots and of the men denied access to them, is a frequent consequence.

While there has been considerable discussion of institutional celibacy as a familial strategy, insufficient attention has been given to the means by which institutions maintain and reinforce the sacrifice on the part of individuals. It might make sense, in inclusive-fitness terms, for a family to deposit its “excess” sons and daughters in a monastery or convent, but what compels these individuals, many of whom must be presumed to have normal sexual appetites, to go along? As Tuzin (1995:259) notes, “millions of people through the ages have eagerly risked life, limb, property, freedom, tranquillity, family, reputation, happiness, have even accepted sure and eternal damnation, all for the attainment, not of offspring, but of sexual pleasure.” Some attempt must be made to explain the willingness of individuals, notwithstanding institutional power and familial pressure, to forgo reproduction and its powerful proximate motivators. The specific means through which institutional power is wielded are clearly important, particularly where membership in celibate institutions is voluntary.

Key to an exploration of this issue is the understanding that institutions often have a strong interest in combating individual tendencies to help families reproductively or materially. First, institutions gain if the time and energy of members are not diluted by the demands of offspring and other close kin. Thus, says Paul, “The unmarried man is busy with the Lord’s affairs, concerned with pleasing the Lord; but the married man is busy with this world’s demands and occupied with pleasing his wife” (Cor. 7:32–33; see Chohij 1989:201). Not surprisingly, Cardinal Stickler (de Valk 1990:4) found organizational loyalty to be an explicit motive for celibacy in the early church. A second benefit of celibacy for institutions is that it can help ensure the control of wealth. In many cases, members not only accumulate resources for the organization as part of their duties but also are encouraged or required to donate their own. Many “greedy institutions” (Coser 1974:6) therefore exert pres-

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sure to weaken, eliminate, or prevent kin ties that might create material conflicts of interest for members. Here too, control of sexual relations is often key (Balch 1985).

THE KIN-CUE MANIPULATION MODEL

Gary Johnson (1986, 1989; Johnson, Ratwik, and Sawyer 1987) has suggested that human nonkin altruism may be elicited through socialization by the manipulation of the means individuals use to identify kin. It is not kinship itself that humans recognize but "environmental cues that have typically been highly correlated with kinship" (Johnson, Ratwik, and Sawyer 1987:157). Because these cues are indirect, errors—and manipulation—can occur. Johnson discusses the use of kin terms, association, and phenotypic similarity as the cues most likely to be utilized in kin recognition and suggests that their manipulation in military contexts can elicit volunteerism, risking one's life in combat, and even altruistic suicide. The rhetoric of enlistment in many armed forces is replete with kin terms like "mother country," "fatherland," and "brothers-in-arms"; recruits are typically trained in extremely close physical association, and those who serve together are made to resemble each other as much as possible by means of uniforms, identical haircuts, and the like. Balch (1985) discusses the historical control of reproduction, labor, and resources in nonkin organizations through abduction, enslavement, castration, and forced celibacy. He similarly refers to association and the use of kinship-evoking language and symbolism as typical organizational practices (see also Coser 1974, Crippen and Machalek 1989, van den Berghe 1981).

As is suggested by Johnson (1986), the biological literature on kin recognition mechanisms (Alexander 1990, Hamilton 1964, Hepper 1991, Sherman and Holmes 1985, Wells 1987, Wilson 1987) supports the possibility of manipulated human altruism. In many species, including primates, "kin recognition of one kind or another has been implicated in most kinds of social behavior" (Wilson 1987:9; see also Colgan 1983:39–50; Emlen 1997; Trivers 1985:169–202). Manipulation of kin cues too is relatively common, both between and within species (e.g., Petrie and Møller 1991; see also Conner 1995). In humans, association and phenotypic matching are indeed the most likely cues to apply (Wells 1987). Support for the importance of association is found in studies of Israeli kibbutzim, where children reared together tend to avoid each other sexually (Shepher 1971, Talmon 1964), and Taiwanese arranged marriages, where the practice of rearing children together and forcing them to marry often results in sexual dissatisfaction (Wolf 1995). Less direct evidence is available for other societies (Brown 1991:118–29; Wolf 1995:423–38). It is also possible that the widespread practice of separating children 5 to 15 years of age helps render the opposite sex unfamiliar so that each can "rediscover" the other romantically (Morris 1997:136–37).

The human brain appears particularly prepared to discriminate among human faces, which supports the im-

portance of phenotypic similarity in kin recognition (Gauthier and Logothetis 2000; Wilson 1987; Zebrowitz 1997:23–26). And while much of the literature on uniforms focuses on differentiating members of a society (e.g., Joseph 1986), more obvious (and so perhaps less discussed) is their role in masking differentiation, reinforcing commitment (Arthur 1999), and thus perhaps triggering altruistic responses.

Humans also categorize relationships through language and other symbolic referents. As summarized by Daly, Salmon, and Wilson (1997), all societies exhibit ego-centered kinship terminology based on parent-offspring relationships and distinguish genders, generations, and degrees of relatedness. Further, in all societies kin terms are "metaphorically extended . . . for evocative and propagandistic purposes" (p. 287) to apply to non-relatives and even abstract entities. For some, the ubiquity of fictive kinship terminology and the variability in kinship systems indicate a lack of relationship between cultural and biological notions of kinship (e.g., Sahlins 1976, Schneider 1984), but, as van den Berghe (1979:9) makes clear, "all cultural models of kinship and marriage bear a demonstrable relationship to [biological facts], although not all societies have equally accurate models of the biology of reproduction and heredity." Individuals are generally aware of the genetic relationships that underlie kin nomenclature (Alexander 1979; Chagnon 1979, 1988).

Two additional factors relevant to kin recognition potentially contribute to its manipulation. One is age. Research on first-language acquisition (e.g., Hurford 1991), nonverbal communication (e.g., Klaus et al. 1972), and other aspects of early socialization (e.g., Belsky, Steinberg, and Draper 1991, Draper and Harpending 1988) demonstrates that learning in many domains involves development-related sensitive periods. Thus individuals might be most susceptible to kinship manipulation during childhood. The second factor is attachment behavior. Research has shown that severed attachments are easily replaced with new, stable ones (Ainsworth 1977, Dantas et al. 1985, Sagi et al. 1985). This suggests that new attachments may be easier to make and maintain if existing ones are eliminated. Thus institutions may more effectively reinforce nonkin attachments if they sever or restrict kin relations.

The literature briefly summarized above suggests that because celibacy in institutionalized settings is an altruistic act for the primary benefit of nonkin, it can be facilitated and reinforced through the manipulation of kin recognition cues. Several traits are likely to lead to manipulated kin-cue recognition in institutions. Manipulators should (1) encourage close association that replicates natural kin contexts (particularly parent/child and sibling relationships); (2) encourage the use of false phenotypic matches (uniforms, emblems, hairstyles, speech patterns, mannerisms, etc.) among members of the non-kin association; (3) encourage the use of linguistic and other symbolic kin referents among members of the non-kin association; (4) attempt to obtain young, impressionable recruits; and (5) discourage association with ac-

tual kin. Where voluntary vows of lifelong celibacy occur with direct benefits to nonkin (even if there are indirect benefits to kin), the manipulation of kin-cue recognition cues (as operationalized above) should occur as well.

CROSS-CULTURAL SUPPORT

A review of the historical and ethnographic literature on the major religious institutions suggests that benefits to nonkin, as well as most or all of the predicted kin-cue manipulation traits, are present in these institutions (see Qirko 1998 for expanded discussion). Monastic orders, whether Buddhist, Christian, or Hindu, tend to exhibit the same general pattern. Recruits are immature, usually adolescent but sometimes much younger. For example, infants were often “dedicated” to medieval Christian monasteries (Knowles 1963), and the “small novices” in Burmese Buddhism were typically age 8 or younger (Spiro 1970). Both physically and symbolically (through the renunciation of kinship ties), recruits are separated from blood relatives to live with other members. Even Hindu and Jain orders characterized by seasonal wandering separate members from kin and lay communities, as they frown upon association with nonmembers or wandering alone (Dundas 1992:131). All orders typically demand uniform clothing, hairstyles, and accouterments from members, even where there is no contact with outsiders and social identification is not an issue. In Buddhist monasticism, for example, clothing is typically so similar that robes must be “disfigured” by means of a small dark dot to make them individually identifiable (Horner 1982:407). Finally, the use of kin terms and other symbols of kinship permeates daily life. Leaders and deities are “fathers” or “mothers,” and members are “brothers” or “sisters.” Family names are renounced and replaced with new, shared surnames, and ascetic “lineages” are sometimes meticulously traced back for generations (e.g., Dazey 1990).

Of course, the typical pattern will not necessarily apply to all of the many sects and branches within these institutions. However, even when a particular monastic branch rebels against mainstream institutional values, the practices associated with kin-cue manipulation appear to remain. For example, Yalman (1962) describes the Tapasa order in Ceylon, whose members claim to be returning to the original teachings of the Buddha by renouncing the world and the typical life of Buddhist monks and priests. Yet they too live communally, dress uniformly, and ask their novices to renounce kin and adopt new names.

Celibate offshoots of pronatalist religions such as the Essenes (Baumgarten 1998, Vermes and Goodman 1989), Islamic dervish groups (Karamustafa 1994), and Protestant communal organizations such as the Shakers (Kitch 1989, Muncy 1973) also exhibit the predicted institutional practices. These organizations, even the most ascetic, are highly organized and successful with respect to the acquisition of resources from recruits and nonmembers, whether through alms, payment, gifts, patron-

age, land grants, or in other ways. Thus they have much to gain by demanding and reinforcing vows of celibacy.

To test the value of the kin-cue manipulation model for understanding celibacy in nonkin settings, a systematic analysis of cross-cultural data from a variety of societies was conducted. Of the 186 societies Murdock and White's (1969) Standard Cross-Cultural Sample (SCCS), 108 are identified as sufficiently well described by sources in the Human Relations Area Files for cross-cultural comparisons. In these cases, references to celibacy were sought through the use of HRAF topic codes pertaining to Celibacy (code number 589), Asceticism (785), Holy Men (792), Priesthood (793), Sexuality (831), and General Sex Restrictions (834). For societies with no HRAF files or files judged insufficient by Murdock and White (78 societies), works by authorities recommended by Murdock and White were consulted where possible. In addition, following Betzig (1986), sources listed in bibliographies of a series of papers with standard cross-cultural codes were also examined (Barry and Paxson 1971, Broude and Greene 1976, Murdock and Morrow 1970, Murdock and Provost 1973, Murdock and Wilson 1972, Tuden and Marshall 1972; also Cohen 1969). More recent sources that I judged satisfactory were consulted as well. Every attempt was made to restrict analyses to times and groups delimited by Murdock and White. In 10 cases, pertinent data were considered insufficient, and these societies were dropped from the sample. [For a complete list of sample societies and sources, see the appendix in the electronic edition of this issue on the journal's web page.]

In the sample of societies examined, celibacy is described by sources as (1) absent (10% of sample societies), (2) idiosyncratic (i.e., noninstitutional; 28%), or (3) highly unlikely to exist in institutional contexts (i.e., the presence of celibacy was not reported; 44%). In 32 societies (18%), celibacy is described by sources as present in institutional contexts. That is, following Delamater (1987), it is accompanied by specific roles and statuses that go beyond a descriptive term for individuals who are not married or who abstain from sex (e.g., “spinsters”). In several societies, more than one institutional context for permanent abstinence was found. For example, the Gheg of Albania (48) exhibit three such contexts: becoming a “sworn virgin,” joining the Bektashi order of dervishes, or becoming a Christian priest or monk. Thus, although reported in 32 sample societies, celibacy is actually present in 41 different institutional contexts. It is this number that was used to test the prediction that institutional celibacy in nonkin settings should be accompanied by kin-cue manipulation.

The 41 institutional contexts in which permanent abstinence is reported reveal three distinct patterns (see tables 1 and 2). In 9 cases, celibacy occurs in an *individual* context, that is, where direct benefits of the act, if any, accrue only to the celibates themselves. Celibates play an acknowledged societal role, but there is no evidence of recruitment, apparent altruism, or institutions in the organizational sense of the term. Examples include the *eniglani* among the Ingalik of the Yukon (122), in-

TABLE 1
Celibacy: Patterning of Kin-Cue Manipulation in 41 Societal Contexts

SCCS#	Name	Context	Celibate Group (If Nonkin)	Close Association	Phenotypic Similarity	Kin Symbolism	Young Recruits	Separation from Kin
16	Tiv	I						
18	Fon	N	Amazons	+	+	+	+	+
37	Amhara	N	Christian	+	+	+	+	+
45	Babylonians	N	Priestesses	+	-	+	+	+
48a	Gheg	K	Sworn virgin	-	-	-	+	-
48b	Gheg	N	Bektashi	+	+	+	+	+
48c	Gheg	N	Christian	+	+	+	+	+
49a	Romans	N	Christian	-	-	-	-	+
49b	Romans	N	Vestal virgins	+	+	-	+	+
49c	Romans	N	Cynics	+	+	-	-	+
50a	Basques	K						
50b	Basques	N	Christian	+	+	+	+	+
51a	Irish	K						
51b	Irish	N	Christian	+	+	+	+	+
52	Lapps	K						
54	Russians	N	Christians	+	+	+	+	+
56	Armenians	N	Christians	+	+	+	+	+
66	Khalka Mongols	N	Buddhist	+	+	+	+	+
68	Lepcha	N	Buddhist	-	+	-	+	-
71	Burmese	N	Buddhist	+	+	+	+	+
73a	Vietnamese	K						
73b	Vietnamese	N	Buddhist	-	-	-	-	-
75	Khmer	N	Buddhist	+	+	+	+	+
76	Siamese	N	Buddhist	+	+	+	+	+
100	Tikopia	K						
101	Pentecost	I						
105	Marquesans	I						
111	Palauans	I						
114	Chinese	N	Buddhist	+	+	+	+	+
116a	Koreans	N	Buddhist	+	+	+	+	+
116b	Koreans	N	Christianity	+	+	+	+	+
117	Japanese	N	Buddhism	+	+	+	+	+
122	Ingalik	I						
138	Klamath	I						
141	Hidatsa	I						
142	Pawnee	N	Children of Iruska	+	+	+	+	-
146	Natchez	I						
148	Chiricahua Apache	I						
153	Aztec	N	Priests	+	+	+	+	+
160	Haitians	N	Christian	+	+	+	+	+
171	Inca	N	Virgins of Sun	+	+	+	+	+

NOTE: I, individual; K, kin; N, nonkin. Empty cells in columns 5–9 mean no mention in sources; this does not necessarily mean trait is absent.

dividuals who withdraw from their community and never marry (Osgood 1959:62), and *korongs*, persons who claim direct contact with gods in pre-European Palau (111) (Barnett 1949). None of the predicted traits are reported by sources in these contexts. Celibates, while they may make vows in some cases, do not appear to be aided in their sacrifice through the manipulation of kin cues. In 6 *kin* contexts, delayed or permanently rejected reproduction seems to provide direct benefits primarily or exclusively to relatives. Examples include Basque (50a), Irish (51a), and Lapp (52a) villages, where later-born sons and sometimes daughters postpone marriage, in many cases remaining permanently single. Here, too, sources (e.g., Caro Baroja 1958, Arensberg and Kimball 1968, Paine 1965, respectively) provide no evidence that kin-cue manipulation takes place. Celibates live not in close

association with one another but with their families. No fictive kin terms or markers such as uniform dress or hairstyles are employed. There is one exception: Gheg sworn virgins are recruited, typically by their fathers, as youths (Coon 1950).

In the preceding 15 cases, celibate behavior, however institutionalized, does not appear to provide direct benefits to nonkin, and reinforcement of celibate vows through kin-cue manipulation does not appear to occur. However, in 26 cases, institutionalized celibacy appears to take place primarily in *nonkin* contexts. Christianity and Buddhism are represented in 18 of these, and all aspects of the kin-cue-manipulation model are found in each of these specific instances. The one exception is the Lepchas of Sikkim (68), among whom lamas often marry and close association and the use of kin symbolism do

not appear to be present (Gorer 1967). The other 8 cases of permanent abstinence in nonkin, altruistic settings are the Bektashi dervishes in northern Albania (48b), "the Amazons" of the Kingdom of Dahomey around the turn of the 19th century (18), the priestesses of Babylon around 1750 B.C. (45), the Vestal Virgins of Rome around A.D. 100 (49b), the Cynics, also in 1st-century Rome, a group of wandering renunciants patterned after Hindu ascetics (49c), the Children of Iruska, a Pawnee association of young "contraries" (142), the Aztec priestly class (153), and the Inca "Virgins of the Sun" (171).

In all of these cases, except the short-lived Pawnee contrary society (Murie 1916:580–81), institutions amass and protect resources through the altruistic service of their celibate members. The predicted pattern appears to apply in these cases as well. For example, Vestal Virgins, chosen by Rome's highest priest to serve for 30 years, were picked at an early age, usually between 6 and 10. Physically and legally separated from their families, they wore distinctive hairstyles and white "bridal" dresses (Benko 1971:66; see also Abbott 2000:38–39). The 19th-century Amazons of Dahomey, known to the general population as "our mothers" (Skertchly 1874:458) and "wives of the king" (Argyle 1966:63), were typically celibate women chosen from the general populace while very young. Each of the several divisions of Amazons was identified with particular clothing, hairstyles, and names. Members' separation from the populace was complete: even when outside the palace they appeared only in groups, flanked by poles that no male was permitted to cross. The Inca Virgins of the Sun, according to Garcilaso de la Vega (1961), were set apart at age 8 or younger to live in permanent seclusion in the Temple of the Sun in Cuzco. They wore uniform clothing and identifying markers and lived in close association with each other and *mama-cunas* ("a woman who has to perform the duties of a mother"). The Aztecs had thousands of celibate priests who "in no wise were to look upon a woman" (Sahagún 1932:65). They too wore distinctive clothing and hairstyles. With other sons of nobility, at age 6 or younger they went to a special training school known as the Calmecac. They rarely saw their parents, were required to sleep in dormitories, and were subject to very strict discipline. Upon showing religious promise they spent additional years at the Calmecac as novices and junior priests (Soustelle 1962, Thompson 1933). While the use of kin terminology among them is unclear, the emperor and at least some deities were referred to by kinship names.

Several qualifications apply to the interpretation of these findings. Ethnographer error and bias are likely to be present. Sources are also uneven in terms of depth of coverage. Further, because of the lack of precision in ethnographic sources, variables were treated as nominal categories. Reference to a trait was accepted as evidence for its existence, and lack of reference cannot guarantee its absence, however unlikely it may be that researchers overlooked it. The use of the SCCS in this manner is not without precedent, even among evolutionary-minded re-

TABLE 2

Distribution of Kin-Cue Manipulation Practices in Societal Contexts Exhibiting Institutionalized Celibacy

Practice	Nonkin Contexts (<i>n</i> = 26)	Kin and Individual Contexts (<i>n</i> = 15)
Close association	23	0
Phenotypic similarity	23	0
Kin symbolism	21	0
Young recruits	23	1
Separation from kin	23	0

searchers (Betzig 1986, Jankowiak and Fischer 1992), but errors must be presumed to exist.

Another issue is the historical independence of the SCCS sample societies. It is obvious that the major religions have influenced each other's structures and practices and that each is found in many different societal contexts. It could therefore be argued that the cross-cultural presence of the predicted pattern is explained simply by diffusion. However, cultural traits associated with institutions do not necessarily diffuse en masse; their perceived utility and congruence with existing cultural patterns are important factors affecting trait adoption (Barnett 1953, Rogers and Shoemaker 1971, Weinstein 1997). A number of syncretic, nativistic, millenarian, and other religious movements, even within the major religions themselves, demonstrate that structural traits are not necessarily maintained in institutions as they are introduced into new settings (de Waal Malefijt 1968: 329–59; Lanternari 1963; Lewis 1986). Thus one arguing for diffusion would still have to explain why the institutional practices associated with the predicted pattern continue to characterize the major religions in new contexts. In addition, because of cultural distance, diffusion is much more difficult to argue for the other institutional contexts in the sample.

Another potential explanation for the presence of the predicted pattern is that the institutional practices discussed occur for reasons other than kin-cue manipulation. Kanter (1972), for example, argues that in Protestant communal groups (e.g., the Shakers), sexual abstinence and investments of money, time, and labor on the part of members are institutional practices that themselves reinforce commitment, not behaviors requiring reinforcement through kin-cue manipulation or other means. One could also argue that the challenge of organizing large numbers of individuals is sufficient to explain the use of uniforms and other insignias of membership or that kinship terms are used to promote organizational solidarity simply because of their linguistic universality. However, the strength of the kin-cue manipulation model is that the five predicted traits are interrelated elements of an independently derived, logically consistent proposition (Lett 1996). Alternative interpretations do not appear to be better explanations of the predicted traits' collective presence in organizations.

CONCLUSION

More can be done to explore kin-cue manipulation in celibate and other altruistic contexts. One logical step is to use historical sources to trace the development of particular celibate institutions in greater detail. Another is to expand the application of the model to include other organizations that demand altruism from their members. While an obvious example is Johnson's research on military organizations, aspects of the pattern may even apply to organizational life in contexts as disparate as businesses (e.g., Vlahos 1985) and prisons (Giallombardo 1966). A related area of research that deserves increased attention pertains to the source of the manipulation, as it is insufficient to say simply that "organizations" manipulate recruits into altruistic behavior. Finally, additional research on kin-cue mechanisms in humans is clearly needed. For instance, the specific psychological constituents of indoctrinability remain largely unexplored (although see Eibl-Eibesfeldt and Salter 1998).

While recent applications of an evolutionary perspective in biology, anthropology, psychology, and other fields have done much to illuminate human cultural behavior (e.g., Barkow, Cosmides, and Tooby 1992, Betzig 1997, Mithen 1996), perhaps the greatest challenge to this perspective is costly altruism for the benefit of nonkin. What appears to be a puzzle can be better understood when it is shown that the nonkin involved are often institutions maintained in large part through labor and other resources generated by individual members. To survive, these institutions must develop means to combat any tendencies individuals may have to contribute these resources to their kin. The institutionalization of celibacy itself, of course, minimizes the possibility that these kin will be members' own children, but the possibility that members will invest resources in other close kin always exists. Thus, even in cases where inclusive-fitness theory may explain the desire of celibates to aid kin, institutions are likely to attempt to maintain and reinforce celibate behavior. Practices that facilitate identification of nonkin as close genetic relatives, which in turn promotes altruistic behavior with nonkin as the primary beneficiaries, appear to be one means through which this is accomplished.

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Anthropologists' Attitudes Towards the Concept of Race: The Polish Sample¹

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The study of human biological variation has a long, and—because of its scientific complexity and social implications—controversial history (see, e.g., Stanton 1960, Stocking 1968, Stepan 1982, Banton 1987, Barkan 1992, Shipman 1994, Marks 1995, Wolpoff and Caspari 1997, Hull 1998, Štrkalj 2000a). Numerous scientific and non-scientific misunderstandings and controversies still characterize this field of research. One of the main problems which still divides biological anthropologists into at least two groups is whether the concept of race is useful in the study of human variation. This paper presents the results of a survey conducted in Poland which aims to assess the attitudes of biological anthropologists in that country towards the concept of race and specifically towards the existence of human races.

The first systematic research concerning the racial origins and classification of humankind can be traced back to several 16th- and 17th-century treatises. Linnaeus (the founder of modern taxonomy), Buffon, and Blumenbach laid the foundations for the biological classification of *Homo sapiens* in the 18th century. At the same time, the study of racial origins was becoming more and more emancipated from biblical genealogies, and it developed largely in terms of the conflict between proponents of polygenism (separate origins) and monogenism (common origin). Improving anthropometric techniques gave new impetus to studies of human variation. These studies

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were institutionalized by the establishment of anthropology as a scientific discipline in the mid-19th century, with the formation of anthropological societies in Europe.

Research on race was, for a long time, based upon an ethno-geographic and typological approach. Some 19th-century anthropologists identified the notion of an ethnic group with that of race. This tendency, as Stołyhwo (in Jasicki et al. 1962) notes, resulted from their not being fully aware that ethnic groups (distinguished on the basis of linguistic and cultural community) are never homogeneous with regard to their somatic characteristics. Typology gained momentum at the beginning of the 19th century thanks to Cuvier's work (see Banton 1987), reached its peak by the end of that century in classical studies such as Ripley's *The Races of Europe* (1899) and Deniker's *The Races of Man* (1900), and dominated the study of human variation up to the second half of the 20th century. As research progressed and samples got larger, it became almost impossible to classify the majority of the living individuals or skeletal remains analyzed into racial types. This, however, resulted not in the collapse of the typological program but in attempts to refine it, usually by the creation of more types. Physical anthropologists subdivided major races into smaller units (subtypes), thus creating multiplicity of "races," "major races," "small races," "geographical races," "sub-races," "anthropological types," and so forth. European populations were, for example, divided into races such as Nordic, Alpine, Mediterranean, Dinaric, Baltic, etc. The number of proposed races (and eventual further subdivisions) varied from one writer to another. Fierce discussions revolved around the proper division of humankind and ultimately produced little agreement.

In the period between the world wars some anthropologists (e.g., Huxley and Haddon 1936) became dissatisfied with racial typology. It took some time, however, for the majority of scientists to begin the construction of a new approach to the study of human variation. This happened only in the second half of the 20th century, and although external factors (World War II, civil rights movements) played an important role, internal factors seem to have been crucial. The development of modern genetics and the emergence of a synthetic theory of evolution, which brought about "population thinking" (not at all compatible with racial typology), had a tremendous impact on biological anthropology (Mayr 1963). In addition, the concept of a subspecies in biology was questioned (Wilson and Brown 1953), and the discussion also infiltrated the field of anthropology.

As a result, new concepts emerged. On one side were those who argued for the abandonment of the concept of race, claiming that no infraspecific classification of the species *Homo sapiens* was possible. Instead, they opted for the study of the distribution of a particular morphological or genetic trait and/or claimed that the breeding population was the only plausible unit of classification among humans (Livingstone 1962, Brace 1964, Montagu 1964). On the other side were those who thought that the concept of race should be redefined in

terms of population thinking (Boyd 1950; Dobzhansky 1951, 1962; Garn 1962; Newman 1963). A lengthy and intense polemic between representatives of the two approaches produced no agreement on the matter. Furthermore, as Kohn (1995) has recently shown, the typological approach is still being pursued by some anthropologists.

"Ethnic group" (see Huxley and Haddon 1936, Montagu 1942) is now used by some researchers (e.g., Lieberman and Reynolds 1996), although they acknowledge the negative potential of the idea of ethnicity. For Montagu (1942)—a pioneer in rejecting the race concept—the term "ethnic group" was a way to refer to biological populations without the baggage of the race concept. The concept of ethnicity may not, however, be a good alternative for the concept of race. In fact, as Wierciński (1962:11) notes, it has proved to be "an even more dangerous concept, opening possibilities for racist theories about the biological superiority of entire nations or nationalities" (e.g., the nationalistic racism of Nazi Germany). In addition, it confuses biology and culture.

The situation in contemporary studies of human variation is obviously complex, and it therefore seemed useful to try to assess the attitudes of today's anthropologists towards the concept of race. Several enlightening studies of this kind have been conducted in the United States (Lieberman 1968, Littlefield, Lieberman, and Reynolds 1982, Lieberman, Stevenson, and Reynolds 1989, Lieberman and Reynolds 1996, Cartmill 1998). Their results suggest that biological anthropologists are still divided on the matter, with neither side forming a significant majority. However, because previous studies have concentrated on American anthropologists (both biological and cultural) or on articles published in the *American Journal of Physical Anthropology*, it appeared reasonable to continue such research in other countries. Poland was thought to be interesting in this regard not only because it has a long tradition of biological anthropology (and the study of human variability in particular) but also because the great debate on races was initiated largely by the papers of two Polish anthropologists in CURRENT ANTHROPOLOGY in 1962.

The tradition of anthropological typology in Poland lasted for over half a century. It began with the so-called Lvov typological school (later known abroad as the Polish anthropological school), created in 1913 by Jan Czekanowski, one of four Polish physical anthropologists trained at Zurich by R. Martin (see, e.g., Bieliński, Krupiński, and Strzałko 1985). During the 1920s Czekanowski (1928, 1934, 1967) developed a sophisticated and complicated system of racial typology (for a review of Czekanowski's method see also Wierciński 1962). He and his followers (e.g., J. Mydlarski, A. Wanke) believed that a population could be objectively divided into pure and mixed racial types. The strong influence exerted by the Lvov school on anthropology in Poland during the 1920s and 1930s continued during the early postwar years. In fact, Polish fascination with typology grew even stronger in the 1950s, partly because of the isolation of Polish science from the West. During that time, the so-

called comparative morphological school of anthropological typology was founded by I. Michalski at the University of Łódź. The ideological connection with Soviet science was not particularly strong in anthropology. While it was the custom for textbooks (see, e.g., Jasicki et al. 1962) to quote Soviet opinions about human racial variation (e.g., Bunak's ideas), Polish anthropologists consistently employed Czekanowski's typological conception.

The Polish anthropological school gained international publicity in 1962 as a result of the publication of two articles by, respectively, an advocate and an adversary of typology—Wierciński (1962) and Bielicki (1962)—in the same issue of *CURRENT ANTHROPOLOGY* and the international debate that they provoked. This marked the beginning of the end of the long history of typology in Poland, which declined with the deaths of its leaders (Czekanowski and Michalski) in 1965. Soon after that, racial typology was no longer taught as a major university course. Changes in anthropology textbooks occurred later (see Malinowski 1975, 1980).

In order to assess the attitudes of contemporary Polish biological anthropologists towards the concept of race, a questionnaire was designed in which the participants were asked whether they agreed with the statement "There are biological races (meaning subspecies) within the species *Homo sapiens*." Three answers were offered: "yes," "no," and "cannot tell." This question is the same as the one used by Lieberman and his coworkers (1989) except that after "biological races" the words "meaning

subspecies" were inserted in parentheses. Specifying the meaning of the term "race" was considered necessary for clarity, but it has the disadvantage that the results of the present survey cannot be closely compared with those of the American anthropologists.

"Race" has been used with several different meanings in anthropology and biology in general (e.g., Tobias 1985, Molnar 1998), and this has often been a source of confusion. One of the reasons is that the burden of the typological approach is still being felt in contemporary anthropology. In the typological framework that dominated research for such a long time, the term "race," with various suffixes and qualifiers, as noted above, could be applied to any biological entity from an individual to a species. The participants in the survey would therefore have been uncertain which meaning of the term was meant by the author of the questionnaire. The results of a pilot study conducted at the 1998 Dual Congress of the Association for the Study of Human Paleontology and the International Association of Human Biologists (in which the question used by Lieberman and colleagues was employed), corroborate this uncertainty (Štrkalj 2000b). Many of those surveyed then suggested that the question should be clearer about the meaning of the term "race." Furthermore, some of the answers were rather confusing, mainly because of semantic ambiguity. One participant, for example, answered "yes" but added "as social constructs."

It was therefore decided that the term "race" would be defined by identifying its meaning with "subspecies"

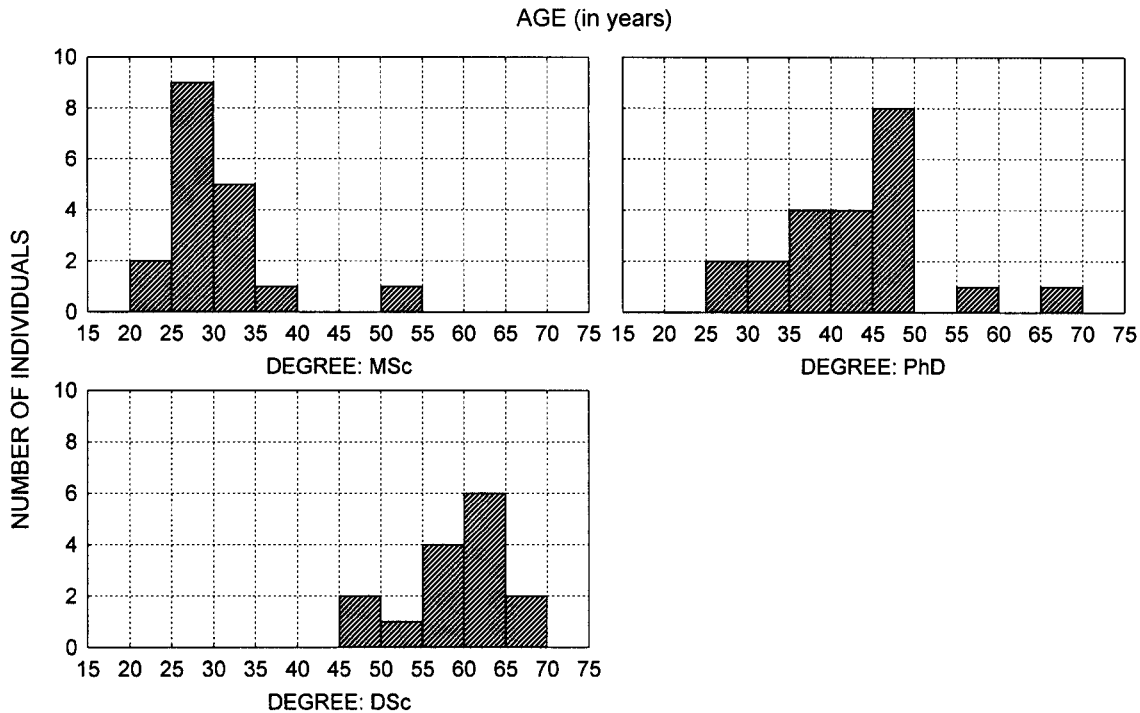


FIG. 1. Age of respondents by highest degree received.

TABLE 1
Age of Respondents by Highest Degree Received

Degree	N	Age (years)			
		Mean	S.D.	Median	Range
M.Sc.	18	30.7	6.76	29.0	24–54
Ph.D.	22	44.1	9.42	45.0	28–68
D.Sc.	15	59.9	5.71	61.0	50–67
Total	55	44.0	13.64	45.0	24–68

(for arguments against the existence of human races as subspecies, however, see Templeton 1998). We considered this important for several reasons. First of all, unlike “race,” “subspecies” is a formal taxonomic category, and therefore its meaning is not vague (Barnicot 1964). Secondly, the majority of recent disputes concerning the application of the concept of race to the study of human variation have concentrated on the concept of race as a subspecies. The major problem has been whether the subspecies concept applies to the study of infraspecific variation. This is obvious from the arguments presented in a seminal paper by Wilson and Brown (1953)—arguments which would later be transferred, with only biologically nonessential additions, to the field of biological anthropology. Yet another reason for identifying race with subspecies in this research is the recent revival of so-called scientific racism (Montagu 1999, Lieberman 2001). Scientists who are prominent in this movement—mainly psychologists interested in IQ testing (e.g., Rushton 1999)—employ the concept of race as a subspecies (usually dividing humanity into three races: African, Caucasian, and Oriental). Because one argument against scientific racism is that the concept of race (as subspecies) used in these studies is invalid, we thought it would be interesting to examine the position of biological anthropologists on this matter. We decided that Lieberman’s statement “There are biological races in the species *Homo sapiens*” was ambiguous, but it must be mentioned that by inserting the word “subspecies” a different ambiguity arises: A respondent who rejects “race” in this particular wording (i.e., as equivalent to a subspecies) might still support “race” in some other of its

many definitions. This possibility and its implications are discussed in the conclusions.

Questionnaires were distributed among biological anthropologists—members of the Polish Anthropological Society (PAS)—at the biennial meetings of the society in Warszawa-Rynia in September 1999. They were distributed to a group in one hall during the general assembly of the PAS after the election of the society’s central board. Eighty people still present in the hall were polled out of about 200 working in the profession (including biological-anthropology Ph.D. students, i.e., those who have completed five years of graduate study and received their M.Sc.). The completion rate was almost 70%. The respondents were employed by various Polish academic or research institutions (or were Ph.D. students) and held at least an M.Sc. degree. They were asked not only to indicate their agreement/disagreement with the statement “There are biological races (meaning subspecies) within the species *Homo sapiens*” but also to give their age, affiliation, and highest degree earned (M.Sc., Ph.D., or habilitation [D.Sc.]).

Before turning to the results, one comment should be made. In this study a dependence was sought between the response and three factors. Two of them—highest degree earned and age—turned out to be especially important. It must be emphasized, however, that among scientists in Poland the highest degree earned and age are highly correlated (Pearson’s correlation coefficient $r = 0.83$; $p < 0.001$), and the dependence, expressed in terms of the results of the analysis of variance, is highly significant: $\phi = 58.4$; $p < 0.001$ (see also fig. 1 and table 1).

In general, out of 55 respondents, 31% agreed, 62% disagreed, and 7% found it hard to tell. The responses broken down by the highest degree received, age, and the employing institution are summarized in table 2 and figures 2–4. Biological anthropologists with an M.Sc. or Ph.D. mostly disagreed with the statement, while respondents with the highest degree, called habilitation (equivalent to D.Sc.), and the status of professor mostly agreed with it (see fig. 2). The correlation between the response and the degree, though not very high, is highly significant ($\chi^2 = 16.7$; $p < 0.01$).

Respondents were grouped into three 15-year intervals: 24–38 years, 39–53 years, and 54–68 years, and accep-

TABLE 2
Proportions of Different Views on Race of Polish Biological Anthropologists by Highest Degree Received, Age, and Home Institution

Response	Total		Highest Degree Received			Age (years)			Institution	
	N	%	M.Sc.	Ph.D.	D.Sc.	24–38	39–53	54–68	University	Other
Agree	17	31	4	4	9	4	6	7	4	13
Disagree	34	62	13	18	3	18	11	5	20	14
Cannot tell	4	7	1	0	3	1	0	3	2	2
Total (N)	55	100	18	22	15	23	17	15	26	29
(%)	100		33	40	27	42	31	27	47	53

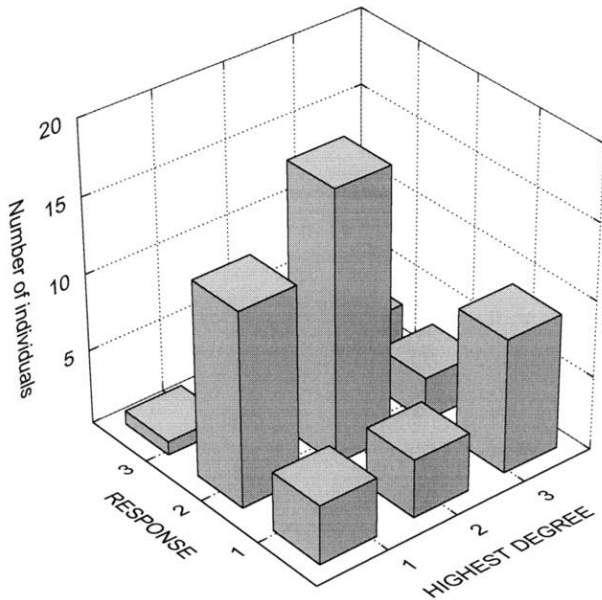


FIG. 2. Responses (1, agree; 2, disagree; 3, cannot tell) by highest degree received (1, M.Sc.; 2, Ph.D.; 3, D.Sc.).

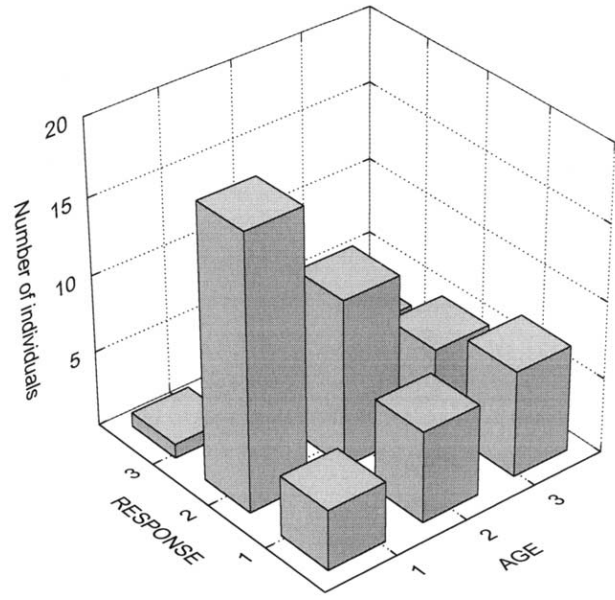


FIG. 3. Responses (1, agree; 2, disagree; 3, cannot tell) by age of respondent (1, 24–38 years; 2, 39–53 years; 3, 54–68 years).

tance of race (as subspecies) clearly increased with age while rejection declines (fig. 3, table 2). The correlation between the response and age is, again, statistically significant ($\chi^2 = 10.5; p < 0.05$). The age of respondents by response is presented in table 3. Although the age ranges for each type of response were basically the same, their means differed significantly ($\phi = 5.92; p < 0.01$). Those who agreed were, on average, more than 50 years old; those who disagreed were, on average, less than 40 years old and those who could not tell more than 55 years old (although this group of respondents was very small). These findings are in agreement with the fact that Polish biological anthropologists born before, during, or shortly after World War II (the oldest generation in this study) grew up with a strong sense of the reality of the race concept. Until roughly the mid-1960s (in some universities even up to the mid-1970s), racial typology was taught as a major university course. As mentioned before, Polish science underwent a long period of isolation due first to the World War II and then to Stalinism and Lysenkoism. In addition, anthropologists of the oldest generation obtained their biological education (biological anthropology was, and still is, merely a specialization in biology) at a time when research in modern genetics was very limited, and this undoubtedly had a bearing on their general views. The education of most of the middle-aged generation of biological anthropologists (i.e., those born in 1946–60) was different (in human biology a population rather than a typological approach to species variation had begun to predominate). Therefore, their perception of race changed, and this was reflected in their views on race.

Apart from the analysis by age and degree, yet another approach was to examine attitudes towards race in relation to the home institution. As mentioned before, biological anthropologists in Poland are employed by various academic and research institutions, including universities, academies of physical education, and medical schools, as well as research centers such as the Polish Academy of Sciences and other, smaller institutions (e.g., museums). Because of the small sample sizes, however, we decided against grouping the respondents into five types of institutions and instead grouped them into two—universities and all others. Such a division is fully justifiable, as universities employ the largest proportion of biological anthropologists (in this study 47% of respondents), who, moreover, are concerned with anthropology in the broadest sense. Figure 4 (see also table 2) reveals that there are clear differences in the respondents' views of race by institution. Those associated with universities reject race five times more frequently than they accept it, while those associated with all other institu-

TABLE 3
Age of Respondents by Response

Response	Age (years)				
	N	Mean	S.D.	Median	Range
Agree	17	50.4	13.85	50.0	26–68
Disagree	34	39.6	11.26	37.0	24–63
Cannot tell	4	55.3	17.56	63.0	29–65
Total	55	44.0	13.63	45.0	24–68

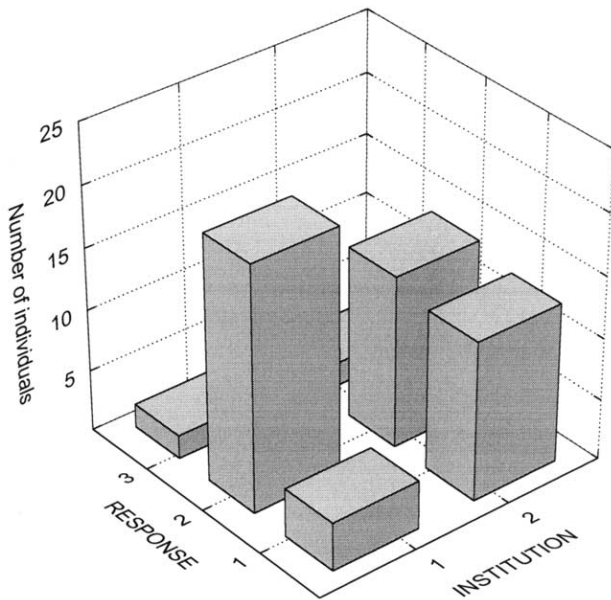


FIG. 4. Responses (1, agree; 2, disagree; 3, cannot tell) by home institution (1, universities; 2, all other academic and research institutions).

tions accept and reject race with almost the same frequency. One explanation of these results may again be differences in age. Anthropologists who are associated with universities are, on average, much younger than those who are associated with all other institutions, and their mean ages (38.5 years and 49.0 years, respectively) differ significantly ($\phi = 9.39$; $p < 0.005$). The correlation between the response (though excluding the third variant, "cannot tell") and the institution was statistically significant ($\chi^2 = 5.67$; $\phi = 0.33$; $p < 0.05$). There was, however, no statistically significant correlation between the type of institution and the highest degree earned.

Although limited because of the factors mentioned earlier, certain comparisons with the American studies (Lieberman, Stevenson, and Reynolds 1989, Lieberman and Reynolds 1996) are possible. For example, younger respondents in Lieberman's studies also rejected race more frequently (see Lieberman and Reynolds 1996). The American surveys showed, in addition, significant differences between the responses of biologists and anthropologists, both physical and cultural. More than two-thirds of responding American biologists accepted the existence of races, as did half of physical anthropologists and less than one-third of cultural anthropologists (Lieberman and Reynolds 1996). In the United States the percentage inclined to abandon the concept of race is the highest among cultural anthropologists, and this is not surprising. The United States has had a long history of racism, strongly expressed in both politics and scientific research. From the conflict of European colonists and indigenous populations through slavery to the recent racial riots in big cities, Americans have experienced traumatic

interactions of what were or still are assumed to be different races. These have repeatedly been interpreted in racism terms by both propagandists and respected academics (Barkan 1992). As a result, Americans have become very sensitive to race, and the term itself has acquired strongly negative connotations. Many American scientists have opted for the nonexistence of human races (see, e.g., Livingstone's [1962:279] declaration "There are no races, there are only clines"). Furthermore, the growing demands of "political correctness" militate against the use of the term in and outside science. As Diamond (1992:96) recently remarked: "Even today, few scientists dare to study racial origins, lest they be branded racists simply for being interested in the problem."

Although not free from traumatic historical experience, Polish society has not experienced such extrascientific pressures concerning race relations. Indeed, in European countries not involved in colonialism, such tensions as have existed have been between different ethnic or religious groups. The most recent, for example, was in the former Yugoslavia, in which biologically very similar-looking but ethnically different groups (cf. Marks 1994) confronted each other in the Western world's bloodiest conflict since World War II.

This study seems to suggest that a greater percentage of biological anthropologists in Poland than in the United States (almost two-thirds versus half) disagree with the statement that there are biological races among living humans, but the main reason for this difference is apparently the different way in which the meaning of the term "race" was construed in the Polish case. In the American survey conducted by Lieberman and his co-workers, the question referred to race in any of its many possible meanings, while race in the present study was identified with subspecies. Bearing in mind the general similarities and differences in responses between Polish and American scientists and the above-mentioned sociopolitical differences between these two groups, one might infer that biological anthropologists in general have increasingly become inclined to abandon the concept of subspecies among living humans, while the concept of race with some other definition might still be accepted. Hence, one could conclude that although there may not be a tendency among biological anthropologists to abandon the concept of race (Cartmill 1998), it seems that such a tendency does exist if race is identified with subspecies.

In the future, the various uses of the term "race" in anthropology should be explored in detail, and surveys should be conducted with clear statements about the term's intended meaning. The differences among biological anthropologists, both within particular countries and in general, may not be as great as they seem. Much of the disagreement may result from semantic confusion.

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